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Wm Tu McDougall

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THE PSYCHOLOGICAL REVIEW

WILLIAM McDOUGALL

The scientific and philosophical contributions of William McDougall are too numerous, too momentous and too wide in scope to be described, much less evaluated, in one article or by any one person. Even if his physiological, anthropological, biological, medical, sociological, philosophical and ethical writings be excluded from consideration, his strictly psychological work, both experimental and theoretical, embraces a range and variety of problems with which probably few persons are fully acquainted. In view of the extent of his productivity it does not seem advisable to give a panoramic picture of his work, but rather to attempt to correct some of the grosser errors in the current "collective representation" of it.

In his remarkable autobiography,[1] McDougall himself alludes ironically to one of these. He remarks that "In America I was known as a writer who flourished in the later middle ages and had written out a list of alleged instincts of the human species." (p. 216).

One of his critics who, unlike others of them, evidently read McDougall's works, has noted the same thing. More than fifteen years ago, before the appearance of the *Outline of Psychology*, the reports on his arduous Lamarckian experiments, the theory of manic-depressive psychosis, numerous other articles and nine or ten books, Dunlap wrote: "I suppose that McDougall has made in total as great a contribution to the science as any living psychologist, and although certain other psychologists have made contributions which are more important than any one of McDougall's, yet the number and

[1] *The history of psychology in autobiography*, (Edited by C. Murchison). Worcester: Clark University Press, 1927, pp. 191–223.

range of contributions would give McDougall high rank. But, as a matter of fact, McDougall is not so well known to the public through his contributions to scientific psychology as he is through the instinct theory which he has elaborated." [2]

It might be noted in passing that the implied distinction between scientific psychology and instinct theory would probably no longer be made. There have been many efforts to dispense with instincts, but every time they are thrown out they come back disguised as 'drives' or 'prepotent reflexes' (F. Allport), as 'desires' (Dunlap), or an 'id' comprising several instincts, as '*echte Bedürfnisse*' (Lewin), or 'appetites' (Tolman). But they no longer evoke apprehension because psychologists have come to recognize, as McDougall without particular ado recognized long ago, that you can not make an explanatory science, whether physics or psychology, *without* constructs. McDougall abhorred vagueness and could not bring himself to leave the hypothetical biological bases of the goals that are common to our species indefinite in number and description. Accordingly he made the assumption (which he showed was empirically plausible) that each instinct (construct) was associated uniquely with a single emotion (phenomenal datum). This assumption provided, in introspective analysis of the emotions, an operation for distinguishing, describing and enumerating the instincts. The assumption may, of course, be rejected if it can be dispensed with, but it is noteworthy that no other clear-cut operation for identifying instincts has ever, to my knowledge, been offered. McDougall was convinced that science was advanced more rapidly by being wrong in so clear a fashion that something got done about it than by being right so vaguely that it had no consequences or testable implications.

McDougall was the most fact-minded and empirical of systematic theorists. Most scientific workers weight one of the criteria of scientific method more than the others and it is fortunate that different temperaments stress different ones. McDougall's primary concern was empirical adequacy, and

[2] Knight Dunlap. *Old and new viewpoints in psychology.* St. Louis: Mosby, 1925, pp. 166. Citation (p. 80) from lecture given in May, 1923.

it behooves anyone who thinks he can dispense with any of McDougall's assumptions to see to it that he covers the tremendous range of behavioral facts against which McDougall continually checked his theory. These facts were drawn from numerous fields, many of them not usually frequented by psychologists. For example, in the chapter on race-making in *Group Mind*, fifteen years before Hrdlicka's findings on the changes in certain anthropometric indices in immigrants to America and their children, he mentions the fact that the French glove industry made gloves of a special shape (long and narrow) for the American trade which were not salable elsewhere.

His scholarship was incredibly comprehensive—never obtrusive, but coloring his every perception. He always read prodigiously, not only in psychology, but in most of the biological fields, in history and in the sciences of man and society; and very widely in philosophy and the physical sciences. The reader of his last book, *The Riddle of Life: a Survey of Theories*, published a few weeks before his death, can not but be impressed by its up-to-the-minute documentation. He searched his own experience and that of others exhaustively for facts to pit against his theory.

McDougall was, moreover, not only the most empirical of systematists but the most systematic of empiricists. Unfortunately the rigorous architectonics of his system are obscured by the literary quality of his writings. He always preferred redefining an old and approximately suitable word in a technical psychological sense to coining a new term. Thus he talked of instincts or propensities instead of an "id," of goals instead of 'incentives,' or sentiments instead of 'mental systems.'[3] This made for better reading but the connotations of the old word were often too potent for the technical definition to be effective with the reader. Thus even some people who have read the *Group Mind* (and multitudes who have not!) believe that it deals with some supernatural or mystical entity or substance whereas it is really a very consistent construct for the explanation of group be-

[3] The concept of sentiments is, by the way, logically more fundamental to his theory as a deductive system than that of instincts.

havior. If he had called his construct, say, a 'social force field' instead of a group mind, doubtless the irrelevant hostility it evoked would have been avoided. Surely the fact that this latter construct is based upon behavioral data while his construct of the individual mind is related to both behavioral and introspective data can hardly explain this curious misconception.

Instances of this sort could be multiplied. He did not number his postulates and definitions as in a geometry, since he was more concerned with pointing to neglected facts and with empirical adequacy in general. But if any one should go to the labor of translating his theory into symbols he would be surprised at its deductive rigor and at the parsimony of its postulates and undefined terms. McDougall could not, as some psychologists have done, be a behaviorist Mondays, Wednesdays and Fridays, when teaching general psychology and a Freudian on Tuesdays, Thursdays and Saturdays when teaching abnormal psychology. Since he saw that neither theory was adequate to the behavioral facts dealt with by the other, this comfortable procedure would have struck him as fundamentally dishonest.

Moreover, from the point of view of scientific method, of pragmaticism in the strict sense, a theory that deliberately excluded large regions of behavior was, to him, sheer trifling. His goal, first, last and always, was to make a *science* of psychology and it was clear to him that no collection of unrelated explanations, whether in terms of 'reflex arcs' or 'Oedipus complexes' could constitute such a science. He sought to develop a set of concepts and 'intervening variables' that would work for all behavior without requiring him to close his eyes to inconvenient facts. He knew too much physiology—and the limitations of our physiological knowledge—to deceive himself with the naive notions of 'pathways' that were current before Lashley; and if the 'intervening variables' required for adequacy to behavorial fact were not compatible with contemporary physical ones, that was nothing to worry about. He had sufficient confidence in scientific method to let the future take care of that. Either physics

would some time be adequate to the required psychological construct, in which case it could readily be translated into the physical terminology of that distant day; or it never would be adequate. He inclined to the latter view or, somewhat less definitely, to Whitehead's, that the unification of psychology (biology) and physics would come about through the biologizing of physics rather than through the physicalizing of biology. In either case the urgent need of the social sciences for a basis in a usable scientific psychology could not wait. For want of it mankind was rapidly destroying itself. One conviction that remained unshaken by his pervasively skeptical temper was that if men were ever to learn to live together in amity and justice it could only be through an understanding of their own nature.

This deeply skeptical temper was most manifest in his questioning of generally accepted views. He held as a matter of principle that when any theory is so widely held that alternative theories are not even respectable, criticism tends to relax and it is high time for a re-examination. He knew from his wide acquaintance with various sciences and their histories that scientists are human, suggestible and subject to waves of fashion just as the rest of their species.

Anyone who has read his illuminating autobiography can not have failed to note its profoundly pessimistic tone. This is especially striking in contrast to the happy, optimistic tone of some of the other autobiographies in the same volume, the achievements of whose authors are objectively, surely, no greater than McDougall's. Such a comparison is, in fact, an interesting illustration of the dependence of the experience of success upon the 'level of aspiration'; if McDougall's goal had been to make a career he could have been more than content. But he set out to make a science of psychology and evidently felt, at the time of writing (about 1928), that he had failed.

It is possible that if rewritten in 1938 it would have been more hopeful. Psychology has come a long way in his direction during the past ten years. His fundamental proposition, that behavior could not be described without reference to

goals, is now so generally accepted as a matter of course that it is hard to realize it was once regarded as a symptom of weak sentimentality! The essence of his theory of mental structure, translated into the less connotative language of geometry, is commanding wide attention. But similar indications had disappointed him before. Thus the opening sentence of his *Physiological Psychology*, published in 1905, is as follows: "Psychology may be best and most comprehensively defined as the positive [as against the normative: ethics] science of the conduct of living creatures." Since the apparent acceptance of this definition issued in the sterile negations of behaviorism, from which psychology (in America) has only just recovered, it is not surprising that he mistrusted even views very closely related to his.

It is perhaps not inappropriate for a colleague and one-time student of McDougall's to note some characteristics that may not be generally known. With all his skepticism he remained extraordinarily alert and hospitable to new ideas clear to the end. He never became rigid as veteran systematists are so likely to become. For example, one of the first, if not the first discussions of Lewin in English appears in his *Modern Materialism and Emergent Evolution*, published early in 1929. That this open-mindedness did not imply any suspension of vigorous criticism is well known.

There was no trace of 'schoolism' in McDougall. He treated his junior colleagues as if they were entirely his equals in age and attainments. The notion of exerting any pressure upon them other than the mutual one of free and equal discussion and argument would have been abhorrent if it could have occurred to him, which it could not. When, a few days before his death, he gave me a copy of his newly published *Riddle of Life*, he remarked with a smile: "I hope the reading of it will land you finally on the side of truth and reason."

His deafness in recent years had made discussion increasingly difficult, but it never made him seclusive or subjective. He regarded it simply as a great nuisance and an external barrier to the social and intellectual intercourse he was accustomed to enjoy.

How he accomplished such a vast amount of work—reading, writing, experimenting and teaching—without ever seeming either rushed or inaccessible is something of a puzzle until one realizes that his work and his life were so much of a piece that he could and did use small bits of time most effectively, resuming work as if no interruption had occurred. He was always thinking psychology. During the last weeks of his life he recorded many observations upon his pain, which he never mentioned even to his family. His notes of this period contained numerous questions and speculations of psychological interest, of which only a few can be cited: Is his Groningen (1926) thesis that sensory pain is a crude form of conative pain tenable? If not, why not? Introspection not likely to suffice. Is there a specific pain center? If so, its discovery might permit of the surgical relief of pain.

As is well known, 'will power' was for him merely the expression of integration of mental structure. Always a patriotic Briton, he was much disturbed by the implications of the British reception of the Munich partition of September 30th and drew an analogy between the collapse of the British group mental structure through fear and what he considered the collapse of his own through pain. He wondered if 'the great inhibitory power of pain is distinct from that of fear.' At the same time he comments upon the relative inhibition of his pain by the effort required to make the notes. It is to be hoped that more of these than can be mentioned here may some time be published.

As a departmental chief he was beyond comparison. When the time came each year to draw up a teaching schedule for the ensuing year, he asked each staff member: "What are you planning to offer next year?" He often spoke with amazement of the 'barbarous' teaching loads common in many institutions and insisted upon a very moderate one for his colleagues. Nor was this accompanied by any slightest pressure for 'output' such as vitiates too much of our literature. He often remarked that psychology was the most difficult and exacting of the sciences and that it demanded the widest preparation and the most reflection and maturity.

Any number of incidents could be used to indicate the quality of the man. His starting a very laborious seventeen-year research on the Lamarckian theory (now in its 50th generation) at the age of fifty is characteristic not only of his courage but of his desire to get the facts and of his faith in experimental method. The fact that eight years later he started a second such project, with adverse selection (now in its 29th generation) to control the possibility of involuntary selection in the first series, although he was satisfied none had occurred, indicates his belief that facts would ultimately overcome even the mightiest of prejudices. His staying with it as long as he could be about for as much as an hour a day in order to introduce his son, Kenneth, to his technique and to devise new check experiments; his more than generous recognition of the work of others; his recommendation as his successor of a psychologist whom he was vigorously criticizing in print—all these and countless others can only suggest his stature as a man.

Professor Yerkes once remarked after reading McDougall's autobiography: "His life is a major tragedy." In the strictest sense of tragedy, this is profoundly true. The frustrations of little men with little goals are not the materials of tragedy. McDougall's frustration of his goal—to make a science of psychology—was.

DONALD K. ADAMS

Duke University

THE PROBLEM OF STIMULUS EQUIVALENCE IN BEHAVIOR THEORY [1]

BY CLARK L. HULL

Institute of Human Relations,
Yale University

Introduction

In his presidential address (**18**, 14), Tolman remarked incidentally that the writer's theory of adaptive behavior contains no explanation of stimulus equivalence.[2] At about the same time, Adams complained (**1**, **10**) in a particular context that the writer has given no definition of the term *stimulus*. In a certain important sense both criticisms involve the same question. Moreover, they represent what has been implicit for a long time in the attacks of *Gestalt Theorie* on behaviorism. These considerations emphasize the fact that the problem of stimulus equivalence is a fundamental one. This is true not only for behaviorism but for any psychology purporting to deal in a thorough-going manner with adaptive behavior. It accordingly deserves the most serious consideration by all schools of psychology.

The problem of stimulus equivalence is essentially this: How can we account for the fact that a stimulus will sometimes evoke a reaction to which it has never been conditioned, *i.e.*, with which it has never been associated? For example, it is evident that a given physical object as sensed by the eye, say, probably never presents the same physical pattern of light energy to the retina on any two occasions. On the other hand, we have the equally well recognized fact that within

[1] The author is indebted to Dr. Donald T. Perkins for the mathematical derivation of equation 7, p. 20 ff.

[2] In the same connection Tolman expresses the belief that there is in the writer's system a corresponding lack of an explanation of response equivalence. It is impossible to agree with Tolman on this point; a serious attempt has been made really to account for numerous phenomena involving response equivalence by means of the mechanism called the *habit-family hierarchy* (**8**).

limits mammalian organisms will react to such an object or situation in substantially the same way, or at least in such a way as to attain the same goal or reinforcing state of affairs (response equivalence) on the different occasions of its presentation. Thus a rat's eyes probably never receive exactly the same combination of physically stimulated points on any two occasions as he approaches the choice point in a simple T-maze, yet the hungry animal will, after a time, learn consistently to turn into the alley which leads to the food.

At the outset it is important to note that there are at least three forms of the phenomenon of stimulus equivalence, according as it is dependent upon one or another of three fairly distinct principles or mechanisms. The first and most obvious mechanism responsible for evoking the same or equivalent reactions is the *partial physical identity* of the stimulus complexes involved. This principle is so obvious and has been so fully elaborated by behavioristic writers that it will not be discussed here (**17**, p. 32; **7**). The second principle of stimulus equivalence we shall call *primary generalization* (Definition 8). This is the empirically determined behavioral phenomenon first described by Pavlov as irradiation (**12**, **3**, **5**). The third principle mediating stimulus equivalence we shall call *secondary generalization* (Definition 9). This term has been chosen because in this case stimulus equivalence is conceived to come about indirectly through the previous widespread conditioning of the stimulus continuum to some other reaction.

In order to proceed effectively to a more detailed consideration of the problem of stimulus equivalence, it is expedient to give an elucidation [3] of a number of terms, some of which have already been employed and all of which will be utilized in a more or less technical manner:

[3] It is recognized that as soon as possible precise definitions should be formulated of most of the critical concepts of the system by means of a limited number of undefined terms, the meanings of which can be no more than elucidated. The technique of symbolic logic promises to be of invaluable aid in this task, as is emphasized by the success attained by J. H. Woodger in the field of biology (**20**). Thanks to the generous collaboration of Professor Woodger, the task of thus laying the formal foundations of the present system has already begun.

1. *Stimulus equivalence:* the equivalence of various stimuli for evoking the (qualitatively) same reaction. Quantitative equivalence is not intended.

2. *Response equivalence:* the equivalence of various action sequences which are alike in that they all serve to attain the same goal, *i.e.*, to bring about the same reinforcing state of affairs. This is believed to be mediated by the habit-family hierarchy (8).

3. *Stimulus object:* the physical origin or critical source of a pattern of energy capable of affecting a sense organ. An example would be a half-inch white die with the usual black spots (Fig. 1).

4. *Potential stimuli:* The infinite number of physically distinct patterns or combinations of physical energy emanating from a stimulus object and capable of affecting a sense organ. An example of such a family of potential stimuli is the infinite number of physically distinct light patterns emanating from an ordinary white die (S_2 S_3, S_4, etc., Fig. 1) corresponding to the infinite number of possible angles. Other examples are the infinite gradations of sizes of retinal image, of intensities of light, shadow, etc.

5. *Stimulus continuum* or *stimulus dimension:* an unbroken ordered sequence of potential stimuli. For example, a stimulus continuum would be the sequence of physical light patterns falling at a given point in space from the white die instanced above (Fig. 1) if it were turned about an axis, *e.g.*, one at right angles to a line extending

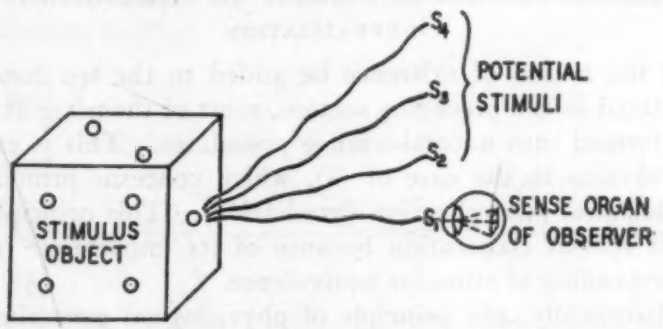


FIG. 1. Diagrammatic representation of a stimulus object, a sheaf of potential stimuli, an actual stimulus (S_1), and the sense organ of the observer upon which the actual stimulus impinges.

from the point to the center of the die. Another stimulus continuum of a far less complicated nature would be the range of simple sound waves from 16 to 30,000 vibrations per second. This latter type of continuum has been called *stimulus dimension* by Boring (4) and Spence (15).

6. *Stimulus* or *actual stimulus:* one of the infinite number of potential stimuli emanating from an object or combination of objects which actually makes an impact upon the sensorium of an organism. An example would be one of the infinite number of possible light-wave patterns emanating from the white die instanced above (Fig. 1), as transformed by the refracting media of the eye, which makes a junction with the system of visual end organs and finally gives rise to a stimulus trace.

7. *Stimulus trace:* an unspecified physiological reverberation within the organism presumed to occur during the impact of the stimulus and to persist for some time after its termination, diminishing to zero with a negative acceleration and capable at any point in its course of becoming conditioned to (associated with) a reaction (9).

8. *Primary* or *physiological generalization* (irradiation): the fact that when a reaction has been conditioned to the trace of a stimulus, the traces of other stimuli from the same stimulus continuum and adjacent to the first will evoke the reaction with an intensity which decreases as the difference between the stimuli increases, the rate of the decrease in the irradiation gradient showing a negative acceleration when the distance from the stimulus conditioned is expressed in units of discrimination thresholds (j.n.d.'s).

9. *Secondary generalization:* the presumptive situation where primary generalization to one or more reactions has been set up to this stimulus continuum and where, if another reaction is then conditioned to a single point on this continuum, all points, however distant, will have a finite excitatory potentiality toward the evocation of this reaction. A somewhat detailed deduction of this phenomenon from lower-level principles is presented in a later section of the present study (pp. 27–28).

10. *Reaction threshold:* the amount of excitatory potential which is required before an overt reaction may be evoked by a stimulus. An illustration of this phenomenon appears in the fact that in most learning situations one or more reinforcing repetitions are necessary before a conditioned stimulus will actually evoke the reaction being conditioned to it (14).

Characteristics of Primary or Physiological Generalization

If the notion of existence be added to the ten concepts elucidated in the preceding section, most of them are at once transformed into natural-science postulates. This is especially obvious in the case of (8), which concerns primary or physiological generalization (irradiation). This principle deserves special elaboration because of its importance in the understanding of stimulus equivalence.

Historically, the principle of physiological generalization has been taken in the main directly from conditioned-reaction investigations and is primarily an empirical finding concerning a relation existing between stimulus continua and reactions originally conditioned to a single point on such a continuum or dimension. Beyond implying that the explanation (theoretical deduction) of this principle, when and if an explanation is achieved, will be physiological, no physiological hypothesis, Pavlovian or otherwise, is intended. In the present systematization it has the status of a behavioral postulate or primitive assumption, with no assumptions as to its origin.

Relevant experimental studies in this field are that of Anrep, working in Pavlov's laboratory (2); that of Bass and Hull (3); and that of Hovland (5). These investigations,

while differing much in the organisms and the experimental conditions employed, and more than one would like in the details of the findings, nevertheless are completely consistent in showing two essential characteristics:

1. If a reaction (R_X) is well conditioned to one point on a stimulus continuum, other points adjacent to the point conditioned will evoke the reaction without further reinforcement.

2. There is a gradient in the intensity or amplitude of these generalized reactions; the more the stimulus evoking the reaction differs from the stimulus originally conditioned to the reaction, the weaker the reaction evoked.

However, a number of important points concerning this type of generalization are not adequately covered as yet by empirical investigation. Some of these will be discussed in the following pages.

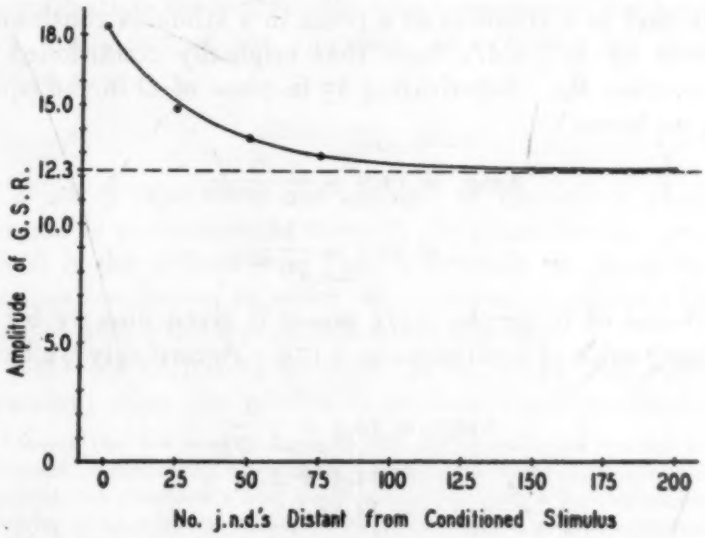


FIG. 2. The curve is a graphic representation of the equation (see text) which has been fitted to the four values of Hovland's physiological generalization gradient of excitatory potential, the latter being represented by the solid circles. Note that if D (the number of j.n.d's) should be increased without limit, the amplitude of the galvanic-skin reaction, according to this equation, could never fall below 12.3. This means that in a situation of this kind all points in the stimulus continuum have already attained a stimulus equivalence through the physiological generalization gradient alone.

Hovland's experimental results (5) on the gradient of generalization of excitatory potential over the continuum of frequency of simple sound waves, uncomplicated by intensity, as manifested by the amplitude of the galvanic-skin reaction, will serve conveniently as a concrete point of departure in discussing primary generalization. His experimental findings are shown by the solid circles in Fig. 2. An equation fitting these values well enough for the present purpose is:

$$\text{Amp.} = 12.3 + \frac{6}{10^{.0135D}},$$

where *Amp.* is the mean amplitude of the galvanic-skin reaction as recorded by Hovland's apparatus and D is the number of discrimination thresholds, or j.n.d.'s on the stimulus continuum separating the stimulus actually conditioned from that evoking the reaction. For example, suppose that the sensorium of an organism should receive the impact of a stimulus at a point in a stimulus continuum differing by 25 j.n.d.'s from that originally conditioned to the reaction R_X. Substituting 25 in place of D in the equation, we have:

$$\begin{aligned}\text{Amp.} &= 12.3 + \frac{6}{10^{.0135\times 25}} \\ &= 12.3 + \frac{6}{10^{.3375}}.\end{aligned}$$

The value of 10 to the .3375 power is given directly by an ordinary table of logarithms as 2.175. Accordingly we have:

$$\begin{aligned}\text{Amp.} &= 12.3 + \frac{6}{2.175} \\ &= 12.3 + 2.75 \\ &= 15.05.\end{aligned}$$

The curve in Fig. 2 is a line drawn through a series of points determined from the equation by a series of such substitutions.

The curved line in Fig. 2 may be seen at a glance to conform to the two characteristics of generalization mentioned above as well established by experiment. In addition it

presents the gradient as one of negative acceleration. This is here provisionally adopted as the basis for the present explorational analysis.[4] Finally, it is evident from an inspection of the equation that such a gradient will never fall below a rather large value, in the present case 12.3. This means that under the conditions of Hovland's experiment the conditioning of a single point at once makes all other points within the particular stimulus continuum qualitatively equivalent in their capacity to evoke the reaction conditioned (R_X), at least to some degree.

That this is true for all continua and for all reactions is to be doubted for the following reasons: it is known (1) that the galvanic-skin reaction is evokable by practically all stimuli even at moderate intensities previous to any overt conditioning whatsoever, and (2) that merely shocking a subject without association of the shock with any point on the conditioned stimulus continuum will greatly heighten such generalization tendency. On this assumption the value of 12.3 drops out of the equation, as probably an artifact, leaving it as

$$\text{Amp.} = \frac{6}{10^{.0135D}}, \qquad (1)$$

in which 6 represents the amount of excitatory potential susceptible to diminution through the generalization process, and D is the difference in j.n.d.'s between the point on the stimulus continuum at which reinforcement occurred and the point at which the test stimulus was applied.

Since the excitatory potential at the point conditioned is dependent upon the number of reinforcements received, it is

[4] Spence (15) has recently suggested that the generalization function follows, throughout the greater part of its course, a negatively accelerated diminution as here postulated, but postulates a brief initial section possessing a positive acceleration. A number of experimental findings seem in an indirect manner to support the hypothesis. According to this view, the failure of Hovland's experiment to show any indications of this preliminary period of positively accelerated diminution might be due to the fact that the experiment did not explore the interval between 0 and 25 j.n.d.'s from the point conditioned. For this reason, among others, the assumption regarding the exact mathematical characteristics of the generalization function represented by equation (1) must be regarded as especially tentative until more adequate evidence is available.

evident that a thoroughgoing working hypothesis of any concrete generalization situation requires a statement of the excitatory potential at the point conditioned (E_c) as a function of the number of repetitions (R). As a first approximation to this relationship we shall assume throughout the present paper the relationship to be [5]

$$E_c = A - \frac{A}{e^{hR}}, \tag{2}$$

where h is a constant dependent in the main upon the relative magnitude of the units selected for E and R, and A is the physiological limit of the organism for acquiring excitatory potential. Thus if $A = 100$, $e = 10$, $h = .04$, and $R = 25$, the equation becomes

$$E_c = 100 - \frac{100}{10^{.04\times 25}}. \tag{3}$$

Solving this equation we find that

$$E_c = 90.$$

With the value of the excitatory potential at the point conditioned at our disposal we may now rewrite equation (1) as follows:

$$E_D = \frac{E_c}{e^{kD}}, \tag{4}$$

[5] It is important to note that while equation (2) purports to be a curve of learning, it must not be confused with any of the numerous curves of learning plotted directly from empirical data. On the contrary, it represents a preliminary attempt to state in quantitative terms the basic excitatory potentiality which is postulated to lie behind the various observable manifestations of learning. Among these are: the decrease in reaction (with continued repetition); the increase in the amplitude or vigor of reaction; the increase in the resistance to extinction; the decrease in the number of errors made; the increase in the number of items recalled; and so on. It is to be noted, further, that equation (2) represents a negatively accelerated learning function. In view of our great ignorance regarding these matters, this equation is put forward very tentatively, largely with a view to calling attention in a concrete and specific manner to the various problems involved. Actually, a number of known facts are consistent with the assumption that the rise of excitatory potential as a function of repetitions, at least during the early stages of learning, may be represented by a straight line. Hull and Perkins (11) have found, for example, excitatory potential (E) to be a very clear linear function of the oscillation or distractibility threshold of rats during compound trial-and-error learning.

where D is the number of j.n.d.'s separating the point on the stimulus continuum conditioned from the point at which the generalization tendency is tested, E_D is the generalized excitatory potential at the point tested, and k is a constant depending in the main upon the units chosen in which to measure E and D. If, now, we take $e = 10$ and $k = .01$ we have as a concrete working equation,

$$E_D = \frac{E_e}{10^{.01D}}, \tag{5}$$

which will serve for illustrative purposes. A double-winged generalization gradient plotted from values computed from equations (2) and (4) appears as the upper curve in Fig. 3.

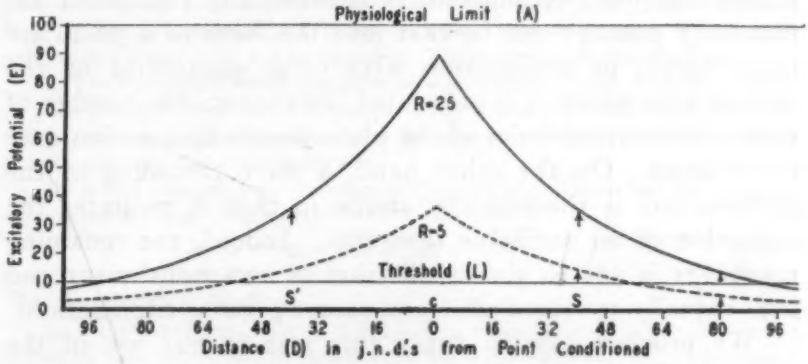


FIG. 3. Diagrammatic representation of two generalization gradients, the upper one based on an original conditioning of 25 reinforcements, and the lower based on 5 reinforcements. The height of each at the point conditioned ($D = 0$) is based on equation (3), whereas that on the respective wings of generalization is based on equation (5). The threshold (L) is assumed to be 10 and the physiological limit (A) is assumed to be 100. The unit of measurement of excitatory potential is assumed to be 1/100 of the distance from zero to the physiological limit.

To the generalization gradient of equation (5) thus arrived at we have only to add the concept of the reaction threshold (see Definition 11). It is accordingly assumed for purposes of concrete illustration that 10 units of excitatory potential (E) are required to overcome the inertia of the reactive mechanism before overt reaction can occur, *i.e.*, that

$$L = 10, \tag{6}$$

where L is the reaction threshold. This hypothetical threshold is represented in Figs. 3, 4, 5, 6, and 7 in relation to various generalization gradients presently to be discussed.

Some Elementary Theorems Based on Primary Generalization

It is more common than one might wish for a writer to rename a well-known phenomenon and thereupon to be credited with having somehow explained it.[6] Accordingly we must inquire whether the present attempt at a solution of the problem of stimulus equivalence is of this fallacious nature. Fortunately there is available a straightforward objective technique for determining whether the renaming fallacy has been committed: A theoretically significant explanatory concept can be cast into the form of a postulate from which, in conjunction with other postulates of the system with which it is associated, an appreciable number of verifiable characteristics of the phenomenon in question may be deduced. On the other hand, a mere renaming of the phenomenon is theoretically sterile in that it mediates the deduction of no verifiable theorems. Indeed, the renaming maneuver is apt to give an illusion of accomplishment and thus actually to retard efforts to secure genuine explanations.

We proceed now to apply this test to our use of the concept of primary or physiological generalization for the explanation of the phenomena of stimulus equivalence:

1. *In a given stimulus dimension the primary generalization gradient may extend in two, and only two, directions.*

This proposition may be illustrated as follows: Let it be assumed that we have given a single stimulus dimension such as the number of sound vibrations per second, other things such as intensity remaining constant. Let it be assumed, further, that some point (c) a considerable number of j.n.d.'s removed from either extreme has been conditioned, in a particular organism, to the reaction R_X by 25 reinforcements. We have already seen (p. 16) that by equation (3) this number of reinforcements yields an E_e of 90. Suppose that, later, the sensorium of this organism receives the impact of S, a point on the same stimulus dimension 40 j.n.d.'s removed from c in the direction of a more rapid vibration. Substituting 90 in place of E_e, and 40 in place of D in

[6] Perhaps as good an example of this type of confusion as any is that associated with the *Gestalt* concept known as *Einsicht*. In this connection see Spence's criticism (16, 214).

equation (5), we obtain for the excitatory potential to the evocation of R_X at S a value of 35.7 (Fig. 3). Suppose, next, the test is made at point S', 40 j.n.d.'s distant from c but in the direction of *fewer* vibrations per second. Since the value of D is the same as before, substitution in equation (5) yields the same value for E_D, *i.e.*, 35.7. Thus we see the stimulus gradient extending symmetrically in two directions.

2. *Many stimulus objects possess generalization gradients extending in an infinite number of directions.*

Suppose we have the stimulus object represented in Fig. 1, and the subject is conditioned to S_1 as there shown. Now it is evident that this die may be rotated about an infinite number of axes, each of which will present to the subject's eye a different continuum of physically distinct stimulus combinations each with its own series of j.n.d.'s and so on, and consequently each with its own gradient of generalization.

3. *The farther removed, in j.n.d.'s of the stimulus continuum concerned, a test stimulus is from the point conditioned, the smaller will be its excitatory potential.*

Now, let us assume that the organism conditioned as in (1) later receives the impact of S'' (Fig. 3, the S'' not shown), a point on the same continuum as S, but 80 j.n.d.'s removed from c, the point previously conditioned to R_X. Substituting 80 in equation (5), we obtain a value for E_D in this case of 14.2 (Fig. 3). It will be recalled from (1) that S, a point 40 j.n.d.'s removed from c, gave an E_D value of 35.7. The value of 14.2 is very much smaller than this. Thus the principle finds illustration.

4. *The excitatory potential of a point on a generalization gradient midway between the point conditioned and some other point will have a value intermediate between the two, and this value will differ from the value at the point conditioned by more than it differs from the value at the more remote test point.*

By further comparing the E_D-values in (3) and Fig. 3, it is evident that 35.7 falls between 90 and 14.2. Moreover,

$$90 - 35.7 > 35.7 - 14.2$$
$$54.3 > 21.5.$$

Thus the principle is illustrated.

5. *The farther removed, in terms of the j.n.d.'s of a stimulus continuum, a test stimulus is from the point conditioned, the weaker will be the reaction evoked.*

There are good experimental grounds (6) for assuming that *the smaller the excitatory potential, the smaller the extent (amplitude) of the associated reaction.* From this and (3) the proposition follows.

6. *The farther removed, in terms of the j.n.d.'s of a stimulus continuum, a test stimulus is from the point conditioned, the longer the latency of the action evoked.*

There are good experimental grounds (14) for assuming that *the weaker the excitatory potential,* the longer the reaction time. From this and (3) the proposition follows.

7. *The farther removed, in terms of the j.n.d.'s of a stimulus continuum, a test stimulus is from a point conditioned, the fewer the unreinforced reactions which will be required to produce experimental extinction.*

There are good experimental grounds (19) for assuming that *the weaker the excitatory potential, the fewer the unreinforced reactions required for experimental extinction.* From this and (3) the proposition follows.

8. *The farther removed, in terms of the j.n.d.'s of a stimulus continuum, a test stimulus is from the point conditioned, the shorter the time required for experimental extinction to a given criterion.*

There are good experimental grounds (19) for assuming that *the weaker the excitatory potential, the shorter the time required for experimental extinction.* From this and (3) the proposition follows.

9. *The fewer the number of reinforcements, the less extended is the supraliminal generalization gradient.*

Let the situation of (1) be assumed, with the exception that the number of reinforcing repetitions is 5 instead of 25. Substituting the 5 in place of R in equation (3), we arrive at 36.9 as the value of E_c. Substituting this value in equation (5), first with the value $D_1 = 40$ and next with the value $D_2 = 80$, we arrive at the two excitatory potentialities 14.6 and 5.8 respectively (Fig. 3). But 5.8 is less than 10, the value of L (equation 6). This means that with only 5 reinforcements a functional generalization gradient does not extend as far as 80 j.n.d.'s from the point conditioned, whereas by (3) with 25 reinforcements the functional gradient extends well beyond this point (Fig. 3). Thus the proposition finds illustration.

10. *The fewer the number of reinforcements, other things equal,*

(*a*) *the weaker the generalized reaction;*
(*b*) *the longer the generalized reaction latency;*
(*c*) *the fewer the number of unreinforced reactions required to extinguish the generalized reaction;*
(*d*) *the shorter the time required to produce extinction of the generalized reaction.*

From reasoning analogous to that following proposition (9), together with principles put forward in connection with propositions (5), (6), (7), and (8), may be derived the preceding propositions, assuming as constant both the number of reinforcements and the number of j.n.d.'s difference between the point tested and that conditioned.[7]

11. *The excitatory potential at a given point within the range of the generalization gradients originating at two distinct points on the same stimulus continuum is equal to the sum of the two generalized excitatory potentials taken separately, less the quotient obtained by dividing the product of the two potentials by the physiological limit of such potentials, i.e.,*

$$E_T = E_{T_c} + E_{T_{c'}} - \frac{E_{T_c} E_{T_{c'}}}{A}. \tag{7}$$

In equation (7), T is the point on the stimulus continuum tested, c and c' are the points originally conditioned, and A is the physiological limit.[8]

Let it be supposed that the same reaction (R_X) is conditioned to two distinct points on a stimulus continuum (Fig. 4) separated by a number of j.n.d.'s less than the sum of the near wings of the super-threshold generalization ranges of the two excitatory tendencies. Let it also be assumed that *overlapping generalization gradients combine in the same manner to produce a joint excitatory potential as would two sets of repetitions each sufficient to produce one of the generalized excitatory potentials in question.*

We have given by equation (2) above,

$$E_c = E_{(R)} = A(1 - e^{-hR}),$$

letting $E_{(R)}$ be the excitatory potential produced at the point conditioned by R rein-

[7] It is to be observed that sets of corollaries parallel to these four follow from propositions 12, 13, 14, 16, 17, 19, 23, 27, and 28.

[8] It is believed that the derivation of the other propositions of the present series is sufficiently obvious to make rigorous proof of them undesirable in this place. Partly as an illustration of what is presumably possible in all cases, and partly because the derivation of the present proposition is more complicated than that of the others, it is here given in some detail.

forcements. We wish to show, first, that

$$E_{(R_1+R_2)} = E_R + E_R - \frac{1}{A} E_{R_1} \times E_R . \tag{8}$$

To prove this we observe that,

$$\begin{aligned} E_{(R_1+R_2)} + \frac{1}{A} E_{(R_1)} \times E_{(R_2)} &= A(1 - e^{-h(R_1+R_2)}) \\ &\quad + A(1 - e^{-hR_1})(1 - e^{-hR_2}) \\ &= A(2 - e^{-hR_1} - e^{-hR_2}) \\ &= E_{(R_1)} + E_{(R_2)}, \end{aligned}$$

from which equation (8) follows.

Now in the case of the generalization gradient tested at point T at a distance D from the point of conditioning, we have by equation (4),

$$E_{T_c} = E_c(D) = E_c e^{-kD}.$$

Substituting in this equation the value of E_c from equation (2) we have

$$E_{T_c} = E(R, D) = Ae^{-kD}(1 - e^{-hR}).$$

Let $\bar{R}$ be the number of repetitions which it *would* take to create E_{T_c} by a direct learning process. Then,

$$E(\bar{R}) = E(R, D).$$

Solving for $\bar{R}$, we have

$$\bar{R} = -\frac{1}{h} \log [1 - e^{-kD}(1 - e^{-hR})].$$

To simplify the notation we shall write this as

$$\bar{R} = f(R, D).$$

Now by previous assumption, the total amount of excitatory potential due to the combination at point T of two generalizations from learning at two points c and c' at distances D and D' from T, after R and R' repetitions respectively, will be

$$E(\bar{R} + \bar{R}') = E[f(R, D) + f(R', D')].$$

Now, by (8),

$$E(\bar{R} + \bar{R}') = E(\bar{R}) + E(\bar{R}') - \frac{1}{A} E(\bar{R}) \cdot E(\bar{R}').$$

Hence,

$$\begin{aligned} E[f(R, D) + f(R', D')] &= E[f(R, D)] + E[f(R', D')] \\ &\quad - \frac{1}{A} E[f(R, D)] \cdot E[f(R', D')], \end{aligned}$$

i.e.,

$$E_T = E_{T_c} + E_{T_{c'}} - \frac{E_{T_c} E_{T_{c'}}}{A} .$$

Q.E.D.

12. *When two points separated by less than half the superthreshold generalization range of either are equally conditioned to the same reaction (R_X), the conditioning of the second will increase the excitatory potential effective at the point first conditioned.*

Now let the situation of (11) be assumed, the two overlapping gradients being 48 j.n.d.'s apart with 10 reinforcements given to each. By equation (3), 10 reinforce-

ments give an E_c of 60.2. The two overlapping gradients are shown in Fig. 4. The generalization gradient of the second at the point S_e, by equation 3, is 19.9. Substituting the values of 60.2 and 19.9 in equation (7) we arrive at a joint value of 68.1 at point S_e. But this is larger by 7.9 than the value originally conditioned.

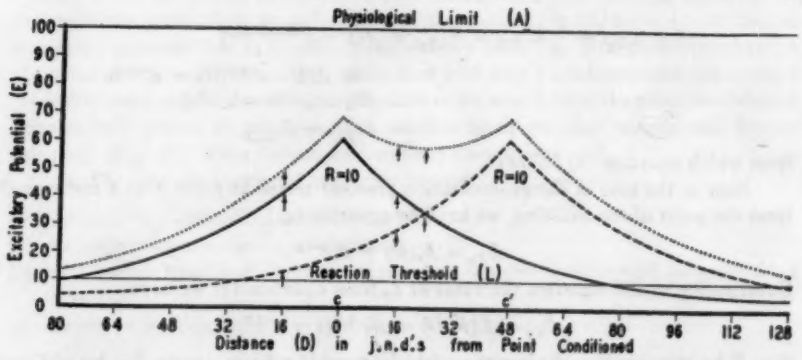


FIG. 4. Diagrammatic representation of two primary generalization gradients set up 48 j.n.d.'s apart, each alike with 10 reinforcements. The dotted line above represents the functional summation of the two overlapping gradients according to equation (7).

13. *When two points separated by less than half the superthreshold generalization range of either are equally conditioned to the same reaction (R_X) the conditioning of the second will increase the excitatory potential effective at points on the far generalization wing of the first.*

Assuming the same situation as in (12), computation shows (Fig. 4) that 16 j.n.d.'s beyond S_e the first gradient has a value of 41.5 and the second one has a value of 13.8. By equation (7) this gives a joint excitatory potential of 49.7, which is larger than 41.5.

14. *When two points separated by less than half the superthreshold range of either are equally conditioned to the same reaction (R_X), the conditioning of the second will increase the excitatory potential effective at points on the near wing of the first by an amount greater than at an equal distance on the far wing.*

Assuming the same situation as in (12), computation shows (Fig. 4) that at a distance of 16 j.n.d.'s from the first toward the second, the first has an excitatory potential of 41.6 and the second has an excitatory potential of 28.7. By equation (7) this gives a joint excitatory potential of 58.4. But $58.4 - 41.6 = 16.8$ and $49.7 - 41.6 = 8.1$, and $16.8 > 8.1$.

15. *When two points separated by less than half the superthreshold generalization range of either are equally conditioned to the same reaction (R_X), the slope of the summation gradient is less than that of the major of the two components but is in the direction of the latter.*

From (12) and (14) we have (Fig. 4), as the difference between the excitatory potential of the major generalization gradient at the point conditioned and at a point 16 j.n.d.'s distant,

$$60.2 - 41.6 = 18.6.$$

The difference between the joint values of the two gradients at the same points is

$$68.1 - 58.4 = 9.7,$$

but

$$9.7 < 18.6.$$

16. *When two points separated by less than half the superthreshold generalization range of either are equally conditioned to the same reaction (R_X) the functional summation values show a minimum at the point of intersection of the near wings of the two gradients.*

The near generalization gradients of the two conditioned parts of Figure 4 intersect 24 j.n.d.'s from each point of origin with an excitatory potential of 34.6 each. This yields a joint excitatory potential of 57.4, which is the lowest point between the two points conditioned.

17. *When two points separated by less than half the mean superthreshold generalization range of either are conditioned to an unequal degree, the joint values from the combining of the near wings of each at a given point of overlapping will show a minimal value at a point nearer the point of weaker reinforcement.*

Suppose one point is conditioned to a stimulus continuum to the extent of 10 repetitions and a second point 48 j.n.d.'s distant is conditioned to the same stimulus continuum to the extent of 5 reinforcements. The primary and summation gradients of this combination are shown in Fig. 5. An inspection of this figure shows that in

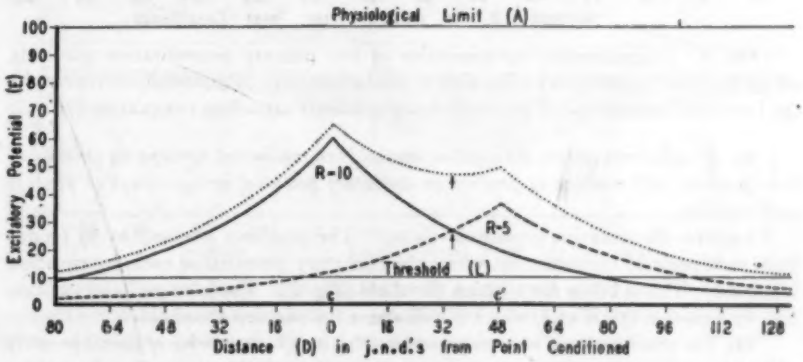


FIG. 5. Diagrammatic representation of the overlapping primary generalization gradients set up 48 j.n.d.'s apart, the left-hand one with 10 reinforcements and the right-hand one with 5 reinforcements. The dotted line above represents the functional summation of these two unequal overlapping primary generalization gradients according to equation (7).

this situation there is also a minimum, but it falls at a point nearer the point of origin of the weaker excitatory potential.

18. *When two points separated by less than half the mean superthreshold generalization range of either are conditioned to an unequal degree, the joint values from the combining of the near wings of each at a given point of overlapping show a steeper slope at the point more strongly conditioned than at the point less strongly conditioned.*

Illustration of this principle appears in the upper curve of Fig. 5.

19. *When a supraliminal generalized excitatory potential overlaps a sublimina generalized excitatory potential, the joint excitatory potential of the two is stronger than the stronger excitatory potential alone.*

Suppose that two points 192 j.n.d.'s apart are conditioned to the same reaction (R_X) each to the extent of 10 reinforcements. The near gradient of one at 48 j.n.d.'s difference from the point conditioned falls on the same stimulus as does the near gradient of the other at 144 j.n.d.'s difference from the point conditioned (Fig. 6). By equation (5) these values yield excitatory potentials of 19.9 and 4.7 respectively, the 4.7 being subliminal since it is well below the threshold value of 10 (equation 6). Combining these by equation (7) we obtain a joint value of 23.7, a gain over 19.9 of 3.8 units (Fig. 6).

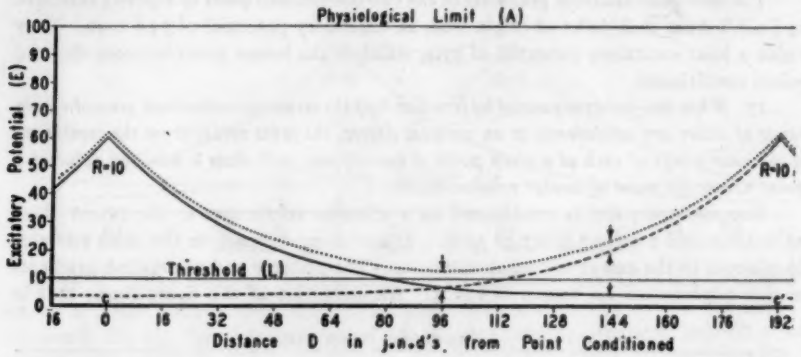


FIG. 6. Diagrammatic representation of two primary generalization gradients set up 192 j.n.d.'s apart, each alike with 10 reinforcements. The dotted line represents the functional summation of the overlapping gradients according to equation (7).

20. *If sufficiently close to the reaction threshold two subliminal overlapping generalization gradients will combine to produce an excitatory potential strong enough to produce overt reaction.*

Suppose the situation presented in (19). The gradients intersect at 96 j.n.d.'s from each point of reinforcement where the excitatory potential of each, by equation (5), is 6.6. This is below the reaction threshold (Fig. 6). The joint excitatory potential, by equation (7), is 12.8, which is well above the reaction threshold.

21. *The conditioning of the same reaction (R_X) at a finite number of points properly placed throughout any stimulus continuum will serve completely to generalize the reaction throughout that stimulus continuum.*

Suppose the situation assumed in (19). An inspection of the resulting excitatory potentials (Fig. 6) shows that the sum of the ranges of the two near superthreshold generalization ranges taken separately is approximately 160 j.n.d.'s, whereas the superthreshold combined generalization inner range is evidently something in excess of 192 j.n.d.'s. Moreover, the combination of the excitatory potential of the inner wing of one with the outer wing of the other (Fig. 6) will evidently also extend slightly the outer ranges of the joint gradient over either single gradient alone. From the preceding it is clear that a finite number of conditioned points so distributed over any stimulus continuum will render it wholly generalized to the reaction so conditioned.

22. *When a generalized excitatory potential is extinguished at a given point until it yields a zero reaction, there is still an appreciable amount of latent excitatory potential (E) at that point and an amount of inhibitory potential (I) is generated at that point equal to the difference between the original excitatory potential and the magnitude of the reaction threshold (L).*

Let it be supposed that a point on a stimulus continuum is conditioned to a reaction (R_X) by 25 reinforcements, as in (1). Substitution in equations (3) and (5) shows that under these circumstances the point conditioned will have an excitatory potential of 90, and a point 32 j.n.d.'s removed from the point of reinforcement will have an excitatory potential of 43.1 units. Now suppose that the stimulus at this latter point is presented repeatedly to the subject without reinforcement until no reaction occurs, when presentations will be discontinued. By Definition 10, this will have brought the excitatory potential to the threshold, which by equation (6) is 10.

There is much reason to postulate that situations of this kind generate an inhibitory potential (I) which generalizes in the same way that the excitatory potential does, *i.e.*, according to equation (5). Now, assuming (Fig. 7) that the inhibitory potential is

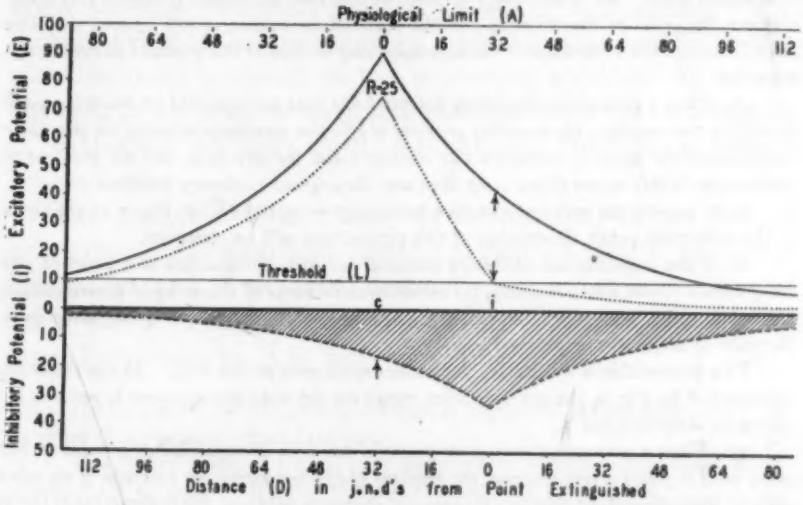


FIG. 7. Diagrammatic representation of a primary positive generalization gradient based on 25 reinforcements and an inhibitory generalization gradient produced by the experimental extinction to the reaction threshold of the generalized excitatory gradient at a point 32 j.n.d.'s from its point of reinforcement. The dotted line between shows the functional summation (effective excitatory potential, or $\bar{E}$) of the positive and negative excitatory potentials according to equation (9).

measured in the same units as the excitatory potential, it follows that the inhibitory potential generated must be the same as the amount of excitatory potential lost, *i.e.*, it will be:

$$43.1 - 10.0 = 33.1.$$

23. *When a generalized excitatory potential is extinguished to the point of zero reaction, the excitatory potential at the point originally conditioned also suffers an appreciable amount of diminution.*

Let us assume the situation of (22). Substituting 33.1 in equation (5) together with a D of 32, we find that the generalized inhibitory potential at a point 32 j.n.d.'s distant in either direction from the point of extinction has an inhibitory value of 15.8. It follows that one of these directions will coincide with the point of original reinforcement.

It is assumed that in situations of this kind the actual effective excitatory potential is given by the equation:

$$\bar{E}_T = E_T - I_T, \tag{9}$$

where $\bar{E}_T$ is the effective excitatory potential at the point tested. Substituting appropriately in equation (9) we have (Fig. 7)

$$\bar{E}_e = 90.0 - 15.8 = 74.2.$$

24. *When a generalized excitatory potential has been extinguished to a point of zero reaction the resulting gradient of effective excitatory potential between the point of conditioning and the point of extinction is steepened.*

By means of computations similar to those outlined in (22) and (23), Fig. 7 has been constructed. By inspecting the curve of effective excitatory potential ($\bar{E}$) lying between the point of conditioning and the point of extinction, it will be seen that the slope is steeper than the slope of the corresponding section of the gradient of excitatory potential.

25. *When a generalized excitatory potential has been extinguished on one wing until it yields a zero reaction, the resulting gradient of effective excitatory potential on that same wing beyond the point of extinction lies entirely below the threshold, and the gradient of diminution in this region is less steep than was the original excitatory gradient.*

If we inspect the curve of effective excitatory potential ($\bar{E}$) in Fig. 7 at the right of the extinction point, illustration of this proposition will be observed.

26. *When a generalized excitatory potential has been extinguished at a point on one wing until it yields a zero reaction, the superthreshold range of this wing of generalization in terms of j.n.d.'s is greatly reduced, depending on how close the point of extinction is to the point of original reinforcement.*

This proposition is a corollary dependent upon proposition (25). In the situation represented by Fig. 7, the generalization range on the wing extinguished is reduced by approximately 60 j.n.d.'s.

27. *When a generalized excitatory potential has been extinguished at a point on one wing until it yields a zero reaction, the gradient of effective excitatory potential of the other wing is lower throughout than was the original excitatory gradient, but the lowering is less at all points than at corresponding points on the wing subjected to extinction.*

The left-hand portion of Fig. 7 illustrates this proposition.

28. *When a generalized excitatory potential has been extinguished on one wing until it yields a zero reaction, the superthreshold portion of the gradient of effective excitatory potential on the other wing is diminished but not to so great an extent as on the wing where the extinction occurred.*

This is a corollary from (27). (See Fig. 7.)

29. *When a generalized excitatory potential has been extinguished on one wing until it yields a zero reaction, the resulting gradient of effective excitatory potential on the other wing is less steep at all points than was the original gradient of generalized excitation.*

The left-hand portion of Fig. 7 illustrates this proposition.

Numerous other propositions could be derived with the aid of the principles associated with the concept of primary generalization. It is believed, however, that the twenty-nine propositions already presented are ample to show that the introduction of this concept to explain one type of stimulus

equivalence is not an empty renaming of the phenomenon in question.

Secondary Generalization

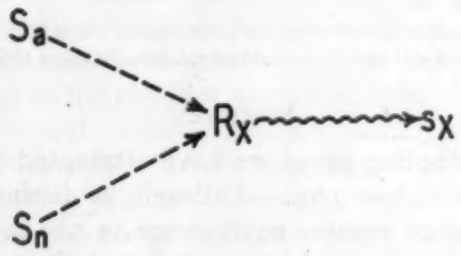
It will be recalled (p. 10) that cases of stimulus equivalence were separated into at least three types, depending upon the presumptive principle or mechanism mediating the equivalence. The fact of partial physical identity of the stimulus was dismissed as not requiring elaboration. The second principle of primary or physiological generalization was discussed in detail in the preceding section. It will be our task in the present section to consider stimulus equivalence as mediated by the third mechanism, that of *secondary generalization*.

The principle of secondary generalization, unlike that of primary or physiological generalization, is not a postulate but is, instead, a theorem derived from other postulates of the system. For that reason a rough outline of the logical procedure whereby it is derived will be given:

1. Let us assume the existence of a stimulus continuum several times wider than the range of any generalization gradient.

2. It is further assumed that at very many places throughout the stimulus continuum of (1) R_X (*e.g.* the blinking of the eyelid) has been conditioned.

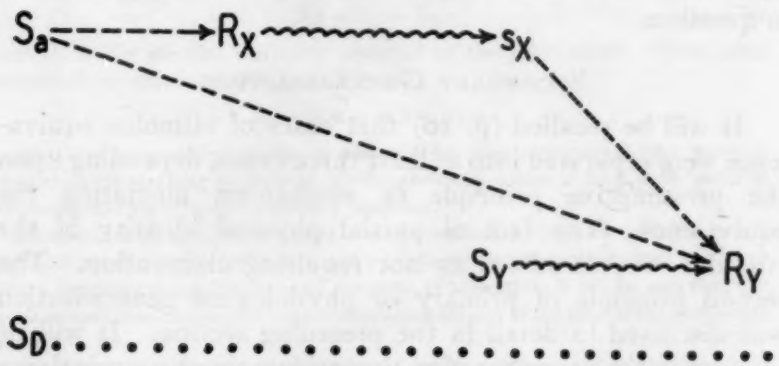
3. From (2) and proposition (21) of the preceding section, it follows that all points throughout this stimulus continuum, whether specifically conditioned to it or not, will evoke R_X to a high and fairly uniform degree; *i.e.*, any two points on the stimulus continuum such as S_a and S_n would evoke R_X to a high and approximately equal degree regardless of the number of j.n.d.'s separating them, thus:



where s_X is a proprioceptive stimulus arising from reaction R_X.

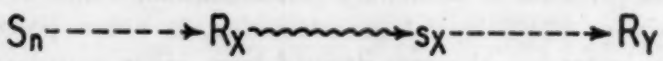
4. Now, let us suppose that a given point, S_a, of this continuum is later conditioned rather strongly to the reaction R_y (*e.g.* a hand movement).

5. From (3) and (4) the process of conditioning would be as follows:



6. Suppose, now, the organism is stimulated by S_n, a number of primary supraliminal generalization spans removed from S_a.

7. From (3), (5), and (6) we have



8. Generalizing (7) we may accordingly say that,

Even though S_n has never been conditioned to R_Y, and even though S_n is beyond the primary generalization range of S_a, nevertheless S_n is able to evoke R_Y, i.e., S_a and S_n are equivalent in their capacity to evoke the same reaction.

Fairly convincing experimental evidence that indirect generalization or equivalence of stimuli really occurs has been reported by Shipley (13). A somewhat detailed analysis of one of Shipley's experimental findings has already been published by the present author (8), and need not be repeated here.

Numerous corollaries flow from the preceding theorem. A sampling of them are:

1. *All points beyond the superthreshold generalization range of the point conditioned to the second reaction will evoke it, but with a weaker intensity than at the point conditioned.*

2. *All points beyond the superthreshold generalization range of the point conditioned to the second reaction will evoke it with approximately an equal intensity, so far as generalization is concerned.*

3. *All points within the superthreshold generalization range of the point conditioned to the second reaction will evoke it to a stronger degree, the closer their proximity to the point conditioned.*

Summary

In the preceding pages we have attempted to show in a number of ways how physical stimuli, as distinguished from stimulus objects, possess equivalence in the sense of being able to evoke qualitatively the same, or equivalent, reactions. *Quantitatively*, reactions from equivalent stimuli are usually weaker when evoked by the stimulus not specifically conditioned (associated).

Three fairly distinct principles or mechanisms have been isolated as mediating stimulus equivalence: (1) the partial physical identity of the stimulus compounds; (2) primary or physiological generalization (Pavlov's 'irradiation'); and (3) secondary or indirect generalization through the arousal of a reaction previously conditioned to the same stimulus continuum. These three forms of stimulus equivalence are conceived to combine their action in various degrees in different situations. Many cases which may appear through our present ignorance to be of the second form may in the end prove physiologically to be of the first. Moreover, the third is dependent upon the second; it is a theorem and not an original postulate. While there probably are other mechanisms mediating stimulus equivalence, it is believed that the three listed above account for most of the behavior properly classed as due to stimulus equivalence.

Incidentally the analysis has enabled us to give what we hope will prove to be a clear and unambiguous elucidation of the term *stimulus*. It is in terms of physical energies such as light waves, sound waves, heat waves, and the like, exactly as these are conceived in the physical sciences. In this way an unambiguous contact of psychology with the physical sciences is established, and its traditional isolation is abolished, at least at this point. At the same time, the traditional confusion of the stimulus with the stimulus object has been definitely clarified. Thus on this count, it is hoped, we have put the behavioristic house in order.

But what of Tolman and the psychologists of various other *Gestalt* groups? How do *they* meet the problem of stimulus equivalence? Surely organisms for them must somehow react to the physical world and, if so, the distinction between the actual stimulus and the stimulus object must exist also. Meanwhile we remind ourselves that merely renaming the thing will not suffice as an explanation however impressive the process of re-christening. Neither will the mere introduction of a non-physical perceptual process between the physical world and the physical reaction solve the problem; consciousness as such no longer possesses this kind

of magic in the scientific world. Despite the monotony of the refrain we must repeat again and again that nothing will serve as scientific explanation except logical deductions from known postulates.

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THE CASE FOR, AND SOME IMPLICATIONS OF THE PLACE THEORY OF HEARING

BY LLOYD A. JEFFRESS

University of Texas

Many writers in the field of audition have presented evidence for the place theory of hearing, or have considered the bearing of a new fact upon it, but usually their discussion has centered around some one aspect of the theory and has not considered the theory as a whole. The present article is an attempt to formulate explicitly a theory of hearing which seems to be held or held in part by many experimenters in the field of audition, and to examine it from the standpoint of the evidence.

The theory holds that the basilar membrane and its associated structures are capable of spatial differentiation between sounds of different frequencies, and that differences in pitch are associated solely with these differences in place of excitation. It holds that the lowest audible sounds affect the portion of the membrane farthest from the base of the cochlea, that the highest audible sounds affect the region near the oval window, and that a complex sound having several components affects as many distinct regions of the membrane between these points as there are distinctly audible components in the sound.

The theory also holds that loudness is a function of the number of nerve fibers innervated by the sound and is not a function of the frequency of the innervation (when the innervation is regular), that the frequency of innervation is, for sounds of moderate and low pitch, an integral quotient of the frequency of the sound, and that the fact that the auditory nerve can conduct frequencies in excess of those predicted from its refractory period can be explained on the basis of the Wever and Bray Volley hypothesis. It holds that within the range of sound-frequencies to which the auditory nerve

responds regularly there is a time relation between the sound and the nerve impulse which makes possible a central-nervous-system representation of any difference in time of stimulation of the two ears and so provides a time basis for localization of sound.

From these basic postulates a number of correlates derive which we will examine as we consider the experimental evidence connected with the theory.

Evidence for Place as the Basis for Pitch

Tonal analysis.—The fact that many of the separate components of a complex tone may be identified has long been recognized and has led to the formulation of Ohm's law of hearing. Ohm's law states that an observer presented with a complex sound hears the tones that correspond approximately to the simple-harmonic components as given by Fourier's analysis. This fact, of course, demands some analyzing mechanism, just such a mechanism as the place theory postulates. The frequency theory has found tonal analysis difficult to account for, and has relegated it to the central nervous system, demanding there a selectivity to frequency for which no physiological basis has been found.

Tonal gaps, pathology, and stimulation deafness.—The fact that there are persons who are less sensitive to some tonal ranges than to others is explicable by the place theory, but has never received a satisfactory explanation by the frequency theory. A post-mortem examination of the cochleas of persons having known abnormalities of hearing should be expected to throw light on this question. However, an examination of the large number of ears (**37**) has yielded results which seem contradictory, some seeming to favor the place theory, others showing lesions to areas of the basilar membrane and other cochlear structures so extensive as to be considered inexplicable in terms of the place theory.

A vast amount of work has been done in the last few years upon deafness resulting from prolonged exposure to intense sound (**6, 7, 8, 17, 18, 35, 37**). Here again the results have been equivocal. Some of the investigators have ob-

tained evidence for the place theory, in that the lesions they found were in those portions of the basilar membrane associated according to the theory with the frequency of the stimulation. Others have found large areas of destruction and losses of hearing covering a wide range of frequencies.

That these apparent discrepancies must be taken as invalidating the place theory does not follow. If we consider some of the implications of the theory we see that from it, and from our knowledge of the acoustics of the cochlea, we might have predicted such contradictory results.

That the ear is not linear in its acoustical response has long been suspected. Helmholtz (**16**, p. 152) suggests that the origin of difference tones and summational tones lies in the non-linearity of the receptor, and lately Stevens and Newman (**29**) and Wever and Bray (**32**, **34**) have shown that the application of a pure tone to the ear gives rise to frequencies in the cochlea, which are harmonics of the generator tone. Newman, Stevens and Davis (**20**) have shown that the application of two pure tones to the ear produces frequencies equal to the difference between the frequencies of the generators, to the sum of their frequencies, and to the various second and third order terms expected from the physical theory. The intensities of these ear-generated tones increase with an increase in the intensity of the sound applied to the ear, and when the sound is of high intensity, at a rate greater than that for the sound. This means, of course, that when we expose the ear to a loud sound we should expect, according to the place theory, to be affecting a number of regions along the basilar membrane proportional to the number of harmonics being generated in the ear. If the sound is of low pitch the number of these regions will be considerable and the regions will extend over a large area of the membrane. If the sound is of high pitch the number will be smaller since the frequencies are multiples of the fundamental frequency; and therefore, for a high-pitched sound these regions will be some distance apart, and the frequencies will soon reach values too high to affect the organ of hearing.

As a result of prolonged exposure of the ear to sound, at least two types of lesion may be expected with both of which the place theory is consistent. Either the receptor cells for the particular frequencies involved may be so damaged or displaced as to be unable to respond to stimulation, or the mechanical structures may be so affected as no longer to respond to the frequencies to which they originally responded. In the first case we should expect to find deafness for the frequencies to which the ear had been exposed, for frequencies slightly above and below this value, since according to the theory a sound affects an area more or less proportional to its loudness, and for similar regions at the various frequencies of the harmonics of the stimulating sound if these overtones are intense enough to cause damage. In the second case we should expect more profound changes in hearing. Since as a result of the damage to the basilar membrane fibers the whole acoustical response of the cochlea will be affected, we may expect parts which were not originally responding to the sound to respond now, to be themselves damaged by the excessive vibration and so on. This would result, finally, in an extent of damage quite out of proportion to the area originally affected by the sound. That both types of lesion will occur together is most likely, and so we should expect that prolonged exposure of the ear to intense sound would result in rather extensive injuries of the cochlear structures, depending upon the frequency of the sound, upon its intensity and upon its duration. The results obtained by investigators in this field seem to be in substantial agreement with these expectations, when we consider that to produce deafness at all, at least in guinea-pigs, a sound 100 db above the human threshold is required (Davis, Derbyshire, Kemp, Lurie, and Upton, 6).

When cochlear lesions less extensive than those produced by intense stimulation are made, deafness to a much smaller range of frequencies results. Held and Kleinknecht (15), using a very fine drill, damaged portions of the external spiral ligament of guinea-pigs and found the subsequent deafness to be very sharply localized. If the injury was slight

the deafness included a region of only one or two whole tones. Stevens, Davis, and Lurie (**28**) using a similar technique obtained cases of deafness for a range of frequencies less than one octave.

Localization of the electrical response.—Culler (**4**), using a modification of the Wever and Bray technique of registering the electrical action of the cochlea, found that the locus along the cochlea of the maximal intensity of electrocal action for a particular sound stimulus was in agreement with the results of other investigators of pitch localization. He found that when the intensity of the stimulating sound was weak the localization was sharp and that with an increase in the intensity of the stimulus there was an increase in the number of places along the cochlea from which electrical responses could be obtained. His map of the cochlea agrees fairly well with that given by Stevens, Davis, and Lurie (**28**).

Loudness of a complex tone.—If loudness is a function of the number of nerve fibers stimulated, any increase in the number should result in an increase in loudness. We should expect, therefore, according to the place theory, that an increase in the number of tones affecting the ear will result in an increase in loudness, and that, probably, two tones of equal intensity will sound louder than one tone having the same total energy. In other words, we should not expect doubling the physical intensity of an audible tone to result in the stimulation of twice as many nerve fibers, whereas we should expect two tones, of sufficiently different pitch to avoid masking, to affect roughly twice as many nerve fibers as one of them will. This conclusion is in substantial agreement with the experimental findings. Fletcher (**13**) found that two tones of equal loudness and sufficiently different frequency appear twice as loud as either one alone, and further that ten tones of equal intensity appear from 20 to 60 db louder than one tone of the same intensity (**12**). Of course, any simple relation that might exist between loudness, intensity and number of components is complicated by masking and by the fact that as the intensity of a tone is increased the number and intensity of 'subjective' tones it creates in the ear are also increased.

Evidence for Place as the only Basis for Pitch

So far, the evidence cited has favored the place theory but has not excluded the possibility that the frequency of the nerve impulse, which many experiments show to be the same as that of the sound for low and moderate frequencies, is one of the factors upon which our judgment of pitch depends. We shall now consider evidence that seems irreconcilable with such an interpretation.

The Wever and Bray effect.—When actions potentials from the auditory nerve are recorded and a careful attempt is made to exclude the microphonic potentials generated in the cochlea, the frequencies obtained do not have values higher than a few thousand cycles per second. That these values (30) exceed those expected from the refractory period of the individual fibers of the nerve is, of course, explained by the Wever and Bray volley hypothesis (31). This means that we must explain the hearing of pitches above these values in terms of some factor other than the frequency of nerve impulse. We might, as Fletcher (11) did, assume that high-pitched sounds depend upon place for their pitch quality, whereas low-pitched sounds depend upon frequency, but we look in vain for any such change in the pitch discrimination function as this would suggest, and there is no such change in the quality of tones, as we proceed from low to high, as this hypothesis would make us suppose.

Pitch as a function of intensity.—If we assume that pitch is a function solely of place, we might expect that, as we increase the intensity of a sound and therefore increase the area of the region of the basilar membrane affected, the disturbance will not spread equally in both directions from the original center. According to this assumption the pitch of the tone may change as we increase its intensity. If, however, the pitch is determined by the frequency of nerve disturbance, which depends solely on the frequency of the sound (for moderate frequencies), the pitch cannot change with a change merely of intensity. Several investigators have noticed such changes and Stevens (24) and Snow (21)

have measured the extent of the change and its relation to frequency and intensity. Stevens found that tones above about 2000~ increase in pitch with intensity, and tones below, decrease. Snow studying tones below 300~ found that they decrease in pitch with an increase in intensity; the greatest change occurred at about 200~ when the sound was very loud and at about 100~ for moderate intensities. These results mean that the pitch of tones must depend upon place alone except possibly for tones below 75~. That very low tones may not depend for their pitch upon the region of maximal disturbance was suggested by Steinberg (**22**) on the possibility that for all low tones the region of maximal stimulation is at the extreme apical end of the basilar membrane. Wever and Bray (**33**) working with very low frequencies of stimulation found difficulty in reconciling their results with the place theory and suggest that the frequency theory is in better agreement with their findings. They seem to neglect, however, the possible role of ear-generated overtones in determining the pitch of tones. This topic will be considered in a later section.

Octave quality.—That two tones an octave apart should sound in some curious way more similar than two tones much nearer together has frequently been cited as evidence for the frequency theory of pitch. The simple arithmetical relation between the nerve frequencies has been suggested as a basis. Fletcher (**12**), however, found that two notes might sound an octave apart when their frequencies were not in the ratio of two to one. Using tones 200~ and 400~ at intensities of 40 db he found that they were matched in pitch by two tones of 222~ and 421~ respectively at intensities of 100 db (shift of pitch with intensity mentioned above). The latter tones seemed to be an octave apart in spite of the fact that their frequencies bore the ratio of 421~ to 222~. This result is not, of course, in accord with the frequency theory.

According to the place theory we should expect two notes an octave apart to sound somewhat alike because they must stimulate many of the same regions of the cochlea, the partials of the higher note being the even-numbered partials of the

lower. Wunderlich (36) has developed a theory of the origin of the musical scale on the basis of this aspect of the place theory.

Binaural pitch comparison.—If the pitch of a tone is determined by the frequency of the nerve disturbance it creates, the two ears should yield the same results in identifying the pitch of a tone. That they do not is well known. If we adjust the frequency of a tone applied to one ear until it matches in pitch the tone applied to the other, we find in some subjects a considerable and fairly consistent difference between the frequencies. That this difference does not result in inharmonious sensations when both ears are hearing the same tone and the fact that the two tones of equal pitch but different frequencies can be heard simultaneously, one with one ear and one with the other without producing a discord, is evidence that dissonance is due to physical beats rather than that consonance results from any kind of fusion of nerve activity.

Number-of-Fibers as Basis for Loudness

Advocates of the place theory usually assume that loudness is dependent either upon the number of fibers innervated or upon the total nerve activity (number and frequency). Forbes' theory of intensity (14) suggests that the frequency of nerve impulse may have a part in determining loudness, and Wever and Bray (31) incorporated this theory in their volley hypothesis.

That increasing the intensity of a tone will increase the frequency with which each nerve fiber will respond, up to a limit set by the absolute refractory period of the fiber or by the frequency of the sound is reasonable to suppose. That such frequency of activity of the fiber, however, is a basis for the intensity of the sensation does not, of course, follow. That it is the only basis for intensity in the case of hearing, as it apparently is in the case of some sensations, has not been suggested.

The work of Davis (5), and of Stevens and Davis (26) bears most directly upon this point. They recorded the

amplitude of the total nerve action of the cat's auditory nerve, when the intensity of sound applied to the cat's ear was kept constant and the frequency varied, and found sudden drops in the amplitude of the nerve response at frequencies that were multiples of the effective refractory period of the nerve. That is to say, they found that at about 900 cycles the amplitude drops to about one-half the value it had at lower frequencies; that at about 1800 cycles it drops to about one-third its low-frequency value; and that at about 2700 cycles there is a still further drop. These drops in amplitude correspond to what we should expect according to the volley hypothesis, and occur at the frequencies at which, according to this hypothesis, we should expect the individual fibers to be unable to respond, say, at every cycle and to respond, therefore, at every other, thus reducing the amplitude one-half; or to be unable to respond at every other and to respond, therefore, at every third cycle, and so reduce the amplitude to one-third its low-frequency value. These experiments furnish excellent substantiation of the volley hypothesis. But they go further and throw considerable light on the problem of intensity. If intensity were dependent, even in part, upon the amplitude of the volley of nerve impulses we should experience a sharp reduction of intensity at those frequencies where there is a reduction of amplitude, and the loudness contours would show sudden drops at those frequencies. No such reductions occur, however, and an examination of the loudness curves shows no sudden drops, but reveals instead a gradual rise with frequency up to frequencies of two or three thousand cycles. Thus we must conclude that the loudness of a sound is not a function of the amplitude of the nerve volleys it generates, but is determined wholly by the number of fibers participating in the volleys.

In the foregoing discussion we have tacitly assumed that the nerve fibers are either participating regularly or not at all. This is not necessarily the case. A fiber located so as to receive only a weak stimulus may respond irregularly and to only an occasional sound pulse. Such a fiber will probably contribute less to the loudness of a sound than one

responding regularly, and so we might expect the number of discriminable loudnesses in a given range to be larger than the number of fibers involved would suggest. This should be the case especially if the frequency is high enough so that the fiber would not have time to recover completely from the previous stimulation. For low frequency sounds we might expect a somewhat greater regularity of response of even the fibers receiving weak stimulation since they will have time to recover completely from the effect of the previous stimulation before the next stimulus arrives. Thus we might expect loudness and pitch to show 'steps' at low frequencies (one 'step' for each new fiber involved) and to show a somewhat more continuous function at higher frequencies. This may be the case. Békésy (**2**) found that loudness and pitch change abruptly with gradual changes of frequency below 50 cycles, but failed to find such 'steps' at higher frequencies. There is not enough evidence in this field to permit drawing any very definite conclusion about the question.

Localization of sound.—As Boring (**3**) pointed out, the fact that we are able to localize a tone on the basis of a difference in phase at the two ears (more strictly speaking, a difference in time) has been one of the strongest arguments for the frequency theory, in that it demands some temporal representation of this difference at some common higher center. With the discovery of the Wever and Bray effect, and with their formulation of a resonance theory incorporating the volley hypothesis (**31**) this argument lost its force. We need merely to assume a correspondence in time between a definite part of the sound wave and its nerve volley to provide the mechanism for this aspect of localization, and such an assumption is in agreement with the experimental facts. Davis (**5**) and Stevens and Davis (**27**, p. 389) have shown that the stimulation of the auditory nerve occurs at a definite part of the cycle, namely at the maximal outward movement of the stapes and tympanic membrane, and Kemp and Robinson (**19**) found that a time difference in the stimulation of the two ears is maintained by the two auditory tracts and can be detected in the lateral lemniscus.

Some Implications of the Place Theory

The basic postulate of the place theory, namely that pitch is determined by place of stimulation, suggests several analogies with other spatially extended sense modalities. Stevens and Davis (**27**, pp. 373–375) discuss a possible similarity between auditory stimulation and visual in the work of Békésy (**1**), *i.e.*, that the stimulation of the ear by a tone affecting a considerable region of the basilar membrane may exhibit a phenomenon analogous to Mach's rings in vision and so provide a definite locus which is dependent upon gradient rather than upon the existence of a maximum.

Another possible analogy has been mentioned by Boring (**3**) who suggested that volume might depend upon the spread of excitation and so be similar in nature to extensity in other senses. Stevens (**23**, **25**, **27**, pp. 163–166) has made out a good case for 'density' as a tonal attribute dependent upon the 'compactness' of the region of stimulation and therefore the inverse of what many have meant by 'volume.'

An additional analogy between hearing and other senses arises when we consider the nature of the pitch-place relationship. Does the pitch-assignment of a given place have the vagueness and variability of some tactual localizations or does it have the definiteness and permanence which we associate with retinal points? Do frequency-pitch assignments for the two ears show the independence of corresponding tactual areas of the two sides of the body or the close relation of corresponding points on the two retinas? These questions suggest possible phenomena to be discovered in the field of hearing.

If the pitch assignment of particular auditory nerve fibers is strict and permanent we should expect certain phenomena to arise in cases of deafness resulting from cochlear injury. If, for example, a certain portion of the basilar membrane is damaged, and the associated receptors, in consequence, cannot be stimulated, the pitches corresponding to this region should be lost. If the physical effect of the damage is not too severe, it is conceivable that the frequencies

originally affecting this region may now affect some other portion of the receptor, and so stimulate a spot once assigned to another frequency and another pitch. This should have the effect, if place is the only determiner of pitch, of permitting the subject to hear as having a certain pitch a tone which originally was associated with a very different pitch. Thus any change in the acoustics of the cochlea might give rise to one or more of these phenomena: gaps in the frequency scale, gaps in the pitch scale, multiple pitch-assignments of certain frequencies, multiple frequency-assignments of certain pitches, and as we shall see later, complex tones whose pitch differs from the pitch of the pure fundamental.

Another consequence of a strict and permanent pitch-place assignment should be a variation, even in normal ears, of the pitch-frequency relation. It is highly unlikely that the locus of stimulation in the cochlea resulting from a given frequency should remain the same indefinitely, or that the two cochlea should remain in strict agreement. We should, therefore, expect to find somewhat different pitches assigned by the two ears to the same frequencies, and that these differences change with time. Whether the pitch differences of the two ears vary with time has not, so far as the writer knows, been investigated.

If pitch-place assignments are not permanent, but are brought about in part by learning, namely such learning as might result from the simultaneous hearing of the same tone by both ears or by the stimulation of the ear by complex tones whose components are in strict pitch relation (octaves, fifths, thirds, etc.), results similar to those mentioned above should be obtained, but during shorter periods of time. We should expect, for example, to find that an agreement for the two ears in pitch would be the modal condition, but that an occasional departure would occur (as a result of a change in one cochlea) followed by a gradual return to agreement. Repeated examination of the same subjects should make it possible to decide between these possibilities. A failure to find any irregularities in the pitch-frequency relationship would argue against the hypothesis that place is the sole determiner of pitch.

Another possible resemblance between hearing and vision is to be found in the analogy between visual patterns and auditory patterns. If we think of the pattern of stimulation of the basilar membrane produced by a complex sound as being roughly analogous to the pattern of stimulation of the retina produced by a frequently seen object we may look for other analogous phenomena. For example, an incomplete visual pattern will be filled in perceptually, as in the completion of shaded letters, or the filling-in of the blind spot. In hearing we might expect the same phenomenon in connection with an incomplete tonal pattern. Just such a completion of pattern is to be found in the hearing of complex tones having certain partials missing. Fletcher (**9**, **10**, pp. 245–250) found that when the fundamental of a complex tone is missing, the pitch of the remaining tone corresponds to the pitch of the missing fundamental, and further that when only a few components remain, the pitch of the combination still corresponds to the frequency of which the components are consecutive multiples. That this phenomenon cannot be explained in terms of difference tones is shown by the fact that when the components presented to the ear have frequencies such as 100, 300, 500, 700, and 900; or 130, 230, 330, etc., the resulting sound has no definite pitch and lacks musical quality.

If the pitch of a complex tone is determined by the pattern it normally produces in the cochlea we might expect the pitch of a complex tone having a certain fundamental frequency to differ from the pitch of a pure tone of that frequency sounded softly enough to generate no audible overtones. This phenomenon should occur, of course, if the region associated with the frequency of the fundamental had changed due to a change in the acoustics of the cochlea. So far as the writer knows this possibility has not been investigated.

A further consequence of the possible dependence of pitch upon the overtone pattern is that the pitch of complex low tones may be below that of the lowest pure tone to which the ear can respond. Just as we can project a pattern into

the blind-spot and 'see' it, so we may be able to project a tone beyond the apical limit of the basilar membrane and hear it. This may be the explanation of the results of Wever and Bray's experiment mentioned above.

Conclusion

The place theory of hearing thus seems to provide a satisfactory account of the phenomena of audition, to escape contradiction by any facts at hand, and to furnish suggestions for further research.

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DIALECTIC IN THE PSYCHOLOGY OF MOTIVATION

BY GEORGENE H. SEWARD

Connecticut College

The growth of the psychology of motivation, like the history of psychology in general, has proceeded by a dialectic process. A 'radical' stand is made; its limitations are seized upon and magnified by a 'reactionary' group which ushers in a counter-revolution; and from these extremes there gradually emerges a more mature middle stand. Without the extremists the compromise would be impossible. Whatever we have achieved so far we owe to earlier theories. These often seem puerile from the more sophisticated perspective of the present and we are apt to forget that they are the *sine qua non* of that perspective. Therefore in surveying the various streams that have fed the main current of contemporary thought in this field we shall seek the constructive contribution of each of them, and at the same time criticize these contributions in the light of current knowledge.

I. Psychological Hedonism

The matrix from which modern theories of motivation have evolved was the doctrine of hedonism. The various forms of this philosophy have been thoroughly sifted by the contemporary hedonist, Troland (**33**). The concept was apparently introduced by the Greek anatomists, according to whom the only individual or social good is happiness. Happiness was variously interpreted by different early schools of hedonism, some giving it a spiritual, others a sensuous meaning. In modern times Hobbes presented a 'psychological' variety, meaning that all human acts are self-interested, involving a search for pleasure and the avoidance of pain. Cartesian influence was reflected in the rational nature of the strivings. Even tendencies later called in-

stinctive were assumed to be rationally directed rather than blindly impulsive. Thus fear was interpreted as the *perception* of pain inherent in an object and causing withdrawal from it.

Troland's originality rests upon his 'law of retroflex action,' which postulates a mechanism to account for the behavior modifications resulting from pleasant or unpleasant after-effects of previous activities. In working out his mechanism Troland takes great pains to tie up the algedonic tone with basic biological drives, neglected by the rational hedonists. According to this view, biologically adaptive and unadaptive acts are accompanied by pleasantness and unpleasantness, respectively. In the course of driven behavior the cortex initiates a variety of responses, some of which will relieve, while others will increase the tension. In either case certain receptors are stimulated which transmit their impulses retroflexly to the cortex. These impulses either facilitate or inhibit the cortical tendency according to whether they are favorable (beneceptive) or unfavorable (nociceptive) to the welfare of the organism. The bene- or nociceptive character of the stimulus is mediated by a subcortical center in the thalamus which registers the individual's organic state. Thus bene- and nociception with their subjective hedonic accompaniments are dependent upon the organism's condition at the moment.

Thorndike's (**26**) recent elaboration of his 'law of effect' shows several points of resemblance to Troland's hypothesis. Like Troland he attributes the operation of the satisfying after-effect to 'some overhead control in the brain' (**26**, p. 41) which constitutes the neural basis of a drive. The two theories differ in their treatment of annoyance. Instead of regarding annoyance as having a primary destructive or weakening effect, paralleling the direct strengthening effect of a reward, Thorndike attributes the apparent weakening to an increased strength of competing tendencies. That is to say, the annoyer causes the individual to make a different response which becomes substituted for the previous unsuccessful one. The chief advance which Thorndike's position

represents over that of Troland is the support he has marshaled for his theory from extensive experimentation in the field of human verbal learning. In so doing he has provided a tool for subjecting the hedonistic principle to experimental analysis.

II. Psychoanalysis

Before Troland announced his modified version of hedonism there had developed in the rich soil of psychopathology a new system of dynamic psychology, psychoanalysis, which clearly showed its hedonistic parentage. According to Freud (7, pp. 308–309), "It seems as if our entire psychological activity were directed toward gaining pleasurable stimulation, toward avoiding painful ones; that it is regulated automatically by the *principle of pleasure*." Freud's hedonism is no longer 'rational,' however, but dependent upon instinctive strivings. This represented an important step in the direction of hormism but was not so complete a capitulation to biology as the term 'instinct' might imply, since for Freud instincts are not clear-cut innate behavior patterns but 'mythical beings superb in their indefiniteness' (12, p. 124). Freud assumed the experience of pleasure to accompany the resolution of tensions. The untrained individual is unbridled in his effort to achieve a direct and immediate consummation of pleasure. In the course of repeated adjustment between his personal wants and the demands of the environment, however, he soon finds it expedient to relinquish present satisfaction in order to insure greater satisfaction in the future. The compromise between internal drive and external necessity is represented by the symbolic 'reality principle.' The struggle between the unbridled desires, especially those with sexual motivation, and the desire to maintain the self in the face of external vicissitudes was stressed by Freud in his earlier teachings (10) as a basic dichotomy in personality. These two aspects of the person are represented as the Id, primitive, unconscious, unsocialized and even unindividualized, and the self-seeking, practical and cautious Ego.

This early schema was soon found inadequate because of

its failure to take account of certain tendencies within the Ego which are so frequently found in the neuroses as to suggest that they may be as fundamental as the others (8). These are tendencies toward aggression against the self and other living organisms. Just as external necessity opposes free expression of libidinous tendencies, so it also opposes and punishes aggressiveness. Moreover the opposition is not confined to external reality. As the individual becomes more socialized the group *mores* are interiorized and an Ego-ideal or Super-ego develops within the organism which serves as the surrogate of the external cultural pattern (11). The tension between this strict Super-ego and the subordinate Ego constitutes a sense of guilt which demands punishment. Thus Freud arrived at his final contrast between the self-preservative and sexual instincts on the one hand, which together are constructive and foster life, and the aggressive and punishing tendencies on the other hand, which lead to destruction and death. The tendency toward death paradoxically becomes in Freudian theory the ultimate goal of life. If pleasure represents the release of the tensions of every-day life, death, in providing the final resolution of all tension, is by inference the supreme pleasure. Seen in this light, death leads toward the reinstatement of a primordial, quiescent, inorganic condition from which organic life was evolved under the influence of external disturbing forces and toward which it is set to return. The self-preservative instincts which constitute an apparent refutation of such an hypothesis are ingeniously made subsidiary to the 'death wish' by being considered the means of securing the path to death peculiar to the organism and of warding off other possibilities of return to the inorganic!

Before attempting a general evaluation of the psychoanalytic contribution to the development of motivation theory we must consider certain major schisms within the movement (9). In Freud's treatment of the relative importance of the Ego and the sexual components the balance was definitely shifted in favor of the sexual, although he admitted a mixture of egotism in all libidinous feeling. To

his distinguished pupil, Adler, the balance seemed to fall in the direction of ego strivings. Around this concept he built his system appropriately called Individual Psychology. Like Freudianism, Adlerian psychology has undergone extensive modifications in the course of its development. In its latest form (1) the basic motive in human life is to win social recognition through the demonstration of superiority in a chosen field of endeavor. All other drives are secondary and important only in so far as they serve this fundamental will to power. In his early childhood the individual adopts a style of life which is manifested in the way he faces each of the three most important problems: love, work, and social life. Each trait reflects the total pattern. Thus personality is not merely the sum of heterogeneous traits but a way of looking at life. If the individual is aware of weaknesses, either organic or social, which might interfere with the attainment of the goal he has set himself, a feeling of inferiority develops which is intolerable. It may lead either to a defeatist attitude or to counteraction by powerful compensatory mechanisms. The latter reaction is considered as more healthy and to it Adler attributes all improvements in the position of mankind. He points out, however, that this constructive effect is possible only in so far as the adjustments are made on the 'useful side of life,' through identification of the ego with social values.

A second major secession from the parent school was led by Jung, who avoided the overemphases of both Freud and Adler by generalizing the libido until it became synonymous with his concept of vital energy (13). This protean force expresses itself now through one, now through another instinctive channel, with the ultimate goal of achieving a state of equilibrium. In accordance with the principle of equivalence, the disappearance of a quantum of libido in one form is followed by the appearance of a corresponding value in another. The manifestations of libido differ not only in kind but also in direction. It may move outward or inward, resulting in two basically different attitudes toward the world, extraversion and introversion, respectively. The further

ramifications of Jung's highly philosophical system lie beyond the scope of this discussion.

Evaluation.—Taking stock of psychoanalytic contributions to the theory of motivation, we find them seriously obscured by philosophical vagueness. This results, first, in a lack of analysis which makes comparison with other motivational systems difficult and uncertain. For example, how shall we relate Freud's formulation of 'life' and 'death' instincts to other organizations? Are they reducible to the more familiar vegetative and emergency drives? Or is the 'death wish' merely a philosophical way of expressing a tendency to regain quiescence regarded by Jung (**13**) and Tolman (**28**) as fundamental to all behavior? This looseness of concept has led to widely differing interpretations of Freudian theory. Although the mystical atmosphere of Jung's teaching contrasts sharply with the practical emphasis of Adler's, they both share the criticism of inadequate analysis. In Jung's 'Analytical' Psychology this appears as a failure to work out the specific instinctive channels through which the libido is expressed, while Adler also fails to present a detailed picture of the operation and interaction of the various drives which mediate his 'will to power.'

A second outcome of conceptual vagueness is ambiguity of terms, with misunderstanding as the inevitable result. Could anything short of a symposium determine the relation between the Freudian and the Adlerian 'ego,' or between the Freudian and the Jungian 'libido'? Indeed, could such a symposium do more than confuse the issue further? Until the concepts can be defined operationally there can be no common meeting ground for investigators in the same field.

Another even more fundamental criticism may be leveled at the absolutism of psychoanalytic systems. In each of the three major branches of the movement the concepts set up are assumed to have universal applicability. The personality pattern is set in early childhood, or even in racial ancestry. Little allowance is made for the effects of changing circumstances of individual life and less for effects of the general societal setting. Recent comparisons among contrasting

cultures are making us increasingly aware of the importance of environmental influences on the so-called basic drives.

Such shortcomings can be avoided only in a system of motivation in which the internal and external factors have been analyzed in specific situations and their specific effects on total behavior pattern determined. In spite of the indefiniteness which obscures psychoanalytic theory at many points, certain concepts have emerged which constitute fruitful hypotheses. Experimentation is already well advanced on such problems as the effect of birth order on the drive for social recognition and the analysis of extra- and introversion. Further attempts to quantify psychoanalytic concepts may be found in the current explorations of Sears (**24, 25**) and Murray (**21**).

III. Purposivism

An important step in bringing motives from behind the veil of philosophy for closer scrutiny was made by McDougall (**17**). With his feet more firmly on the ground prepared by Darwin he was in a better position than Freud to substitute hormism for hedonism. For him the mainsprings of human action are to be sought in a number of specific instincts shared with lower animals. Pleasure and pain are secondary to these biological urges (**18**). Simple sensory experiences which seem to be intrinsically pleasant or unpleasant in reality are dependent for their algedonic tone upon the drive operative at the moment. For example, a sweet taste is pleasant only in so far as it progressively satisfies the impulse to eat. The function of pleasure and pain is to modify instinctive processes, pleasure tending to sustain a given action, pain to curtail it. It is the instincts, however, that are the real sources of action. These are conceived as innate tendencies which are modifiable in both stimulus and response aspects, although the central affective core remains unchanged. In the course of individual experience these prime movers become organized around specific objectives to form *sentiments*, such as love, hate, jealousy and grief. This naturalistic psychology McDougall (**19**)

makes the vehicle of a 'purposive' philosophy, finding in instincts the channels through which operate vitalistic forces.

Evaluation.—Although McDougall's hormic psychology represents a great advance in its attempt to systematize motivation on a biological basis, its influence has undoubtedly been impaired by his philosophical bias. The teleological implications of his theory are not acceptable to the 'tough-minded' laboratory scientist.

Criticism of his psychology has centered around his use of the term 'instinct.' Because of its connotation of innateness, McDougall's departure from the orthodox meaning to include modification through experience was not generally recognized.[1] This ambiguity played a large part in stirring up the 'instinct controversy.' Moreover in listing innate tendencies he failed to provide a basis for their systematic classification. Finally the term led easily to hypostatization. Instincts became *dei ex machina* explanations resulting in circular reasoning reminiscent of 'faculty' psychology. Unchecked, this trend would have produced an unhealthy inertia, blocking experimentation.

IV. Behaviorism

The too comfortable lethargy into which the instinct doctrine threatened to lull psychology was soon to give way to an extreme swing from organism to environment. In a challenging article, "Are there any instincts?" Dunlap (5), questioned the basic assumptions underlying the then current usage of the term. This type of attack led to a more careful analysis of instinct. The first step was a strict pruning down of the concept of instinct to include only unlearned behavior. The Russian literature on reflexology was rich in suggestions along this line. According to Pavlov (**22**) motivation is reducible to reflex action. He eradicated the line of demarcation usually drawn between reflex and instinct. One by one the points of assumed difference are analyzed with

[1] In his later writings (**20**) MacDougall has attempted to meet this criticism by drawing a clearer distinction between the learned and unlearned aspects of instinct. The modifiable phases are dignified to independent status as 'abilities,' which serve as media of expression for the innate 'propensities.'

the discovery in each case of similarities rather than differences. By the time his exposition is completed, however, we find that the concept of 'reflex' has been so broadened as to include not only behavior previously subsumed under the category of 'instinct' but even modes of adjustment commonly considered too complex to fall in that class. For example, he speaks of a *reflex* of purpose, described as 'an aspiration to the attainment of a definite exciting object' (**23**, p. 276). One has the impression that in this system it is not so much that instinct has been reduced to reflex as that reflex has been elaborated to instinct—and beyond.

Like Pavlov, Bechterev (**3**) also preferred to regard instincts as forms of reflex action, which he called 'complex organic reflexes' to distinguish them from the simpler types. Experience is admitted as an influence which molds the direction and expression of an inherited impulse. The mechanism whereby individual experience is conceived to modify innate reflexes is the conditioned response.

The conditioned response became the keynote of Watson's attack on the instinct doctrine. Although in his early work Watson (**34**) admitted a chained reflex concept of instinct in his list of unlearned responses, even this concession to heredity was ruled out of his later writings (**35**). Since he had come to regard 'instinct' as largely the result of conditioning he believed that differences in structure and in early training are sufficient to account for all differences in later behavior. This led to what we may call his 'neo-Jesuit' doctrine: "Give me a dozen healthy infants, well-formed, and my own specified world to bring them up in and I'll guarantee to take any one at random and train him to become any type of specialist" (**35**, p. 82).

The behavioristic viewpoint was carried to its logical extreme in Kuo's 'psychology without heredity' (**14**). This writer proposed that every action be regarded as a forced response describable in terms of specific stimuli. Concepts such as instinct and purpose are for Kuo lazy substitutes for careful and detailed analysis of behavior.

Evaluation.—If scientific progress were to continue in the

study of motivation, analysis obviously had to be carried beyond the categorical thinking of the instinct stage. Behaviorism's fresh attack from the environmental angle opened up new fields of investigation. By manipulation of stimuli the psychologist could now attempt to build into the individual a variety of behavior patterns. Special impetus was given to experiments on animals and children. In their enthusiasm for the environment, however, the behaviorists neglected the organism. Concentrating their attention on the objectively observable behavior units presumably aroused by specific external stimuli, they failed to keep in focus the integrity of the organism as a whole. Simplification had been indicated, but this was oversimplification. Like the small boy who takes the watch apart to see what makes the wheels go around, Behaviorism took the individual apart and left psychology with a number of behavior 'elements' removed from their relationship with the other parts. By overemphasis upon the end-products of molecular analysis it neglected the internal dynamics which gives a molar pattern unpredictable from knowledge of the parts.

V. Field Theories

Out of the controversy between Purposivism and Behaviorism emerged the compromise, 'Purposive Behaviorism,' advanced by Tolman (**28**). This theory represents an attempt to avoid the vitalistic implications of McDougall's 'purpose' and the oversimplification of Watson's 'behavior.' Even in his early (**27**) concept of 'driving adjustments' Tolman admitted the inadequacy of the chained-reflex definition of instinct. In a later development of his theory (**28**) he maintains that, while purpose is not more than what appears in behavior, it is an emergent, characterizing the behavior of the organism as a whole and consequently not to be discovered among its constituent physical and physiological parts. Behavior is purposive in so far as it shows ease of learning with reference to some goal. The basic goal is restoration of the quiescence which was temporarily disturbed by some internal need or external irritant. Pleasure-

pain motivation is secondary to this more fundamental need. Quiescence is achieved differently according to the nature of the specific disturbance. Between a group of independent causal variables represented by stimulus-objects, heredity, training, and others, and the ultimate dependent variable of behavior, Tolman (**30**) postulates a set of 'intervening variables.' Among these are the 'primary demands,' including drives toward biologically desirable objects and away from biologically undesirable situations. On the basis of the primary are developed the 'secondary demands.' For example, in the case of a rat in a maze the primary goal of consummation is mediated by the demand for food and even for the maze alleys (**30**, p. 164). The demands are executed by cognitive functions which take account of differences among qualities and set up hypotheses concerning relations among stimuli. Under the motivating force of one or another demand the individual displays purposive behavior. The degree of purpose depends upon the underlying physiological state. For example, in Tolman's laboratory it has been demonstrated (*6*) that under conditions of hunger motivation maze learning is facilitated more by a food incentive than by water. With decrease of hunger, however, food loses its effectiveness (**32**). Such experiments show the necessity of taking into account both internal and external aspects of the situation in interpreting behavior. Reactions to particular stimuli will differ according to the drive operative at the moment because the very appearance of the objects undergoes concomitant change. Thus stimuli are not constants but forces whose power to evoke responses varies with the organism's condition. They make up a psychobiological rather than a physical field surrounding the individual (**30**).

Closely related to Tolman's general approach is the field theory of Lewin (**15**, **16**). The individual is again conceived of as bearing a dynamic relationship to his environment. The customary distinction between subject and stimulus emphasized by the behaviorist is regarded as false and misleading. Instead of a world of fixed and immutable physical stimuli we are asked to substitute a 'psychological

life space' of which the person and his environment are represented as inextricable parts. This life space is divided into regions whose inner structure may be conveniently described in topological terms. The topology varies from time to time according to changes in the surrounding field and the momentary condition of the organism. Again we may cite the exampie of the different demand value of food for a hungry as compared with a satisfied animal. The environmental objects with their varying demand-values or 'valences' constitute points of origin of driving forces or 'vectors' of attraction or repulsion in the field. Such forces vary in strength and direction. Their strength depends upon internal factors, the presence or absence of other valences, and the physical position of the object with respect to the individual. Direction is given a functional rather than a spatial meaning, since in order to reach a goal it may be necessary to take a spatially circuitous route. Analysis of various reactions to barriers yields information concerning conflict behavior which has important clinical as well as theoretical bearings. According to Lewin's system a description of behavior in concrete immediate situations will reveal dynamic principles with general significance.

Evaluation.—From the dialectic viewpoint the field theories may be regarded as synthesizing the more internal approaches of psychoanalysis and purposivism on the one hand with the external approach of behaviorism on the other. By working out motivation concepts operationally Tolman (**29**) has managed to retain the important contributions of hormism without sacrificing the objectivity of behaviorism. In applying the field theory at the human level Lewin is also able to utilize the advances made by both movements. In agreement with Freud, Lewin would seek the bases of motivation in personality structure. However, instead of deriving his force concepts from biological urges within the person, Lewin derives them from vectors resulting from the interaction between the person and his field. The soul-searchings of Freud and the class concepts resorted to by both Freud and McDougall are abandoned in favor of be-

havior descriptions in immediate situations. The chief advance represented by topological psychology over these hormic approaches may be summarized as greater stress on environment and greater objectivity of method. Lewin, like Tolman, arrives at a kind of operational behaviorism. The static environment of Watson, however, is replaced by a dynamic field of forces and the mechanical robot by an intricately structured organism.

The importance of Tolman and Lewin for the development of motivation theory may be measured by the large number of important research studies they have stimulated. Since Tolman's 'field' is capable of analysis in the familiar stimulus-response terminology, it may be more readily adapted to laboratory techniques. Tolman is very explicit concerning the necessity for working out the effect of separate intervening variables on resultant behavior (31). Lewin's concepts, on the other hand, may prove of greater usefulness in life situations: *e.g.*, nursery school and clinic. In the hands of his pupil, Brown (4), they have already been applied to broader social phenomena. Further appraisal of these formulations must await the results of the research they initiate.

VI. The Future Psychology of Motivation

In tracing the development of contemporary theory we have noted a number of landmarks which determined new directions for investigation. The first major step was the search for a biological basis of motivation, which the traditional hedonism had placed on the lofty pinnacle of reason. The categorical thinking involved in the hormic approach led to a strenuous reaction in the direction of objectivity and piece-meal analysis. From these opposed viewpoints there emerged a new synthesis represented by the field theories, in which the dynamic interdependence of organism and environment are stressed. We are now faced with the question of the chief trends indicated for future development. Our understanding of the dynamics of organism-in-field must be extended by systematic investigation of both organism and field. We need a thorough knowledge of internal factors

such as hormones, vitamines and drugs, which change the thresholds of excitability to certain classes of external stimuli. On the other side of the picture we must manipulate external situations in order to determine their role in the development of modes of perception and attitudes. External factors embrace not only the immediate situation but also those broader influences which make up the cultural pattern. The dichotomy between these two directions of investigation is still in evidence. The anthropological psychologist in the flush of the new worlds which lie before him to conquer loses patience with his biological colleague who keeps his eye to the microscope. Each investigator in following his own bent needs a perspective broad enough to include other approaches. He must remember that such conflicts are the basis of that dialectic by which his science grows and that they lead inevitably to a more mature synthesis. Even now there are indications of that future synthesis. Important though it is to determine the relative contributions of internal and external factors in motivation, the difficult task still remains of explaining the central nervous mechanism which mediates between organism and environment. The organism responds to the environment selectively and this selection is a function of centers in the cerebro-spinal system. Peripheral bodily factors on the one hand and social factors on the other may cause threshold variations in these mechanisms which alter the interpretation of stimulating situations. An understanding of the mechanisms is essential to a thorough understanding of behavior. The time is ripe for further investigations similar to the recent work of Beach (2) on the role of central factors in maternal behavior in the rat. Work in this field has been given its chief impetus by Lashley,[2] the leading experimentalist in brain localization. In so doing he has set a major problem for the future psychology of motivation.

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PHILOSOPHY, PSYCHOLOGY AND POSTULATIONAL TECHNIQUE

BY L. O. KATTSOFF

University of North Carolina

Introduction

It is a strange phenomenon in the contemporary state of all the sciences that the movement that started as an attempt to separate the sciences clearly and distinctly from Philosophy, is turning into an attempt to develop a new fusion between them. And this new fusion is resulting through the analysis of that aspect of the sciences which (as the scientists themselves maintained) was fundamentally distinct from the corresponding aspect of Philosophy—namely in the *realm of methodology*. The new fusion is not merely a return to a previous state of affairs since it finds the sciences and Philosophy greatly altered and greatly benefited by their attempted separation.

Since the revolt of science against systems constructed on *a priori* bases (*i.e.*, with little or no regard for facts except as verifiers of theories) science has tended to say, "Facts speak for themselves." The facts, unfortunately, could not make themselves intelligible, if taken in isolation. But the scientist felt that to put them together would be to set up a *system* and a system smacked too much of precisely the thing against which he was protesting. The result of the revolt against systems without facts seemed to be facts without systems. Obviously, however, this is a matter merely of emphasis. Physics preaching *facts* used Newton's system to discover them. Biology preaching *facts* used either a mechanistic or a form of non-mechanistic system to interpret them. Psychology preaching *facts* used an associationist, behaviorist, topological or some other system to interpret them. True, through these years the various scientific journals reported

experiment after experiment—fact piled on fact—yet somehow despite the tremendous mountain of fact, very little of real value is achieved. The scientist lived in hope—in the hope that some genius might some day establish the factual relations between these. This hope is in process of fulfillment and it is precisely the fulfillment of this hope which is leading to the new fusion between science and philosophy; *i.e.*, to the erection of systematic constructions (**10, 2**).

The entire controversy might be viewed as R. H. Wheeler does (**13**) as an attempt to solve the part-whole problem—or the organismic-atomistic problem.

The development of an organismic or systematistic trend in science arose as a result of three factors:

1. The increasing use of mathematics in scientific procedure. This inevitably leads to synthetic structures.

2. The development of symbolic logic. This gave an implement of precision and also brought about an analysis of methods and the nature of scientific systems.

3. The development of mathematical physics. This gave illustrations of systems in science. It also indicated the tremendous advantage of systematic construction for the purpose of experimentation.

Our interest in this paper will be directed to the second of these factors and in particular to the nature of *scientific systems*.

For many centuries Geometry had used a form of expression which many thinkers praised highly. Descartes had extolled mathematical methods of demonstration and Spinoza had attempted to throw his philosophic system into the form of a Geometry. Recently this form was seen not to be inherently peculiar to mathematics but to be a general form applicable to any region of investigation. The general structure of a postulational system is such that it can be imposed on any body of facts. The advantage of such a system is that it not only gives an orderly arrangement of facts but indicates possible experiments and directions of research.

It is precisely such a system which Clark Hull advocates for introduction into psychological investigation. Clark

Hull calls these systems 'scientific systems' but from his description of them they are clearly what the logician calls 'postulational systems.' There are certain factors involved in the use of such systems which must be considered and which D. K. Adams in his critique of Hull's articles has overlooked (1; *Cf.* 6, 7).

The Postulational System (9)

Every postulational system is composed of two main elements: (1) a set of propositions, (2) an unexpressed method of drawing conclusions. The set of propositions may be further subdivided into (*a*) axioms, (*b*) theorems. The theorems are *derived* from the axioms by the unexpressed method of drawing conclusions. The axioms include all definitions. Since the theorems are derived from the axioms, the system may be considered as resting upon the axioms. There are as a result certain properties which the axioms must satisfy in order to be acceptable. These are three—*Consistency*, *Completeness* and *Independence*. The last of these, *Independence* merely means that no proposition is to be taken as an axiom if it can be deduced as a theorem from the remaining axioms. This condition is obviously not very important so we can omit it from our discussion. It might be of advantage to have before us an abstract, very simple, postulational system and indicate one method of demonstrating the property of consistency for the purposes of comparison with Hull's system.

Given a class C and a relation denoted by $<$.
Let a, b, c be any elements of C.
1. If $a \neq b$, then either $a < b$ or $b > a$.
2. If $a < b$, then $a \neq b$.
3. If $a < b$ and $b < c$, then $a < c$.

The consistency of these postulates is shown by setting up an empirical (preferably experimental) situation in terms of which these symbols can be interpreted.

Demonstration.—Let C be a class of children consisting of a child aged 10, another aged 12 ($= b$), and a third aged 14 ($= c$).

Let $<$ mean "learns nonsense syllables faster than." Then our postulates read

1. If we take any two of the three children, one of them learns nonsense syllables faster than the other.

2. If one child learns nonsense syllables faster than another, they are different children.

3. If one of the children learns nonsense syllables faster than another and this latter faster than the third, then the first child learns the syllables faster than the third.

Consistency is recognized by Hull when he says: "The definitions and postulates of a scientific system should be stated in a clear and unambiguous manner, they should be consistent with one another and they should be of such a nature that they permit rigorous deductions" (**6**).

But Hull does not seem to take this very seriously. As a matter of fact in neither of the two miniature systems presented by Hull, does he take the trouble to exhibit the consistency of his postulates. It is also interesting that Adams in his critique of Hull makes no mention of the omission of this very important matter. Hull himself in his later article (*Mind, Mechanism and Adaptive Behavior*) seems to drop the matter entirely. He merely says, "A satisfactory scientific theory should begin with a set of explicitly stated postulates accompanied by specific or 'operational' definitions of the critical terms employed" (**7**, p. 4, 5).

Reference to our simple illustrative system may indicate a possible reason for Hull's omission. Since he is describing experimental results in his postulates, he may feel that the criterion is satisfied. But it is necessary to show that *all the postulates are satisfied simultaneously*. This Hull has not done even though it may actually be the case.

Consistency means that it is not possible to derive contradictory theorems from the same set of postulates. The importance of this is self-evident. The value of exhibiting the axioms of a given psychological system is greatly diminished if consistency is neglected.

Completeness means that the set of postulates is adequate for the solution of any question which can be constructed in terms of the undefined concepts and definitions in the axioms. This is obviously a very difficult problem. Techniques for solving it are available in the literature of Symbolic Logic (11). Clearly if a meaningful problem can be formulated which is incapable of solution, either affirmatively or negatively, then the set of postulates is inadequate for further work along those lines. Hull makes no mention whatsoever of this property of postulates. If we refer to our illustrative system for nonsense syllables, it will be evident that although the set of postulates given may be consistent, yet one would hardly claim it adequate to answer all problems about nonsense syllable learning. (See below for additional remarks.)

The significance of these remarks lies in the fact that a postulational system may not be any advance if the ambiguities, inconsistencies and inadequacies are taken over into the new system itself.

The erection of *systems* of psychology (*i.e.*, philosophies of psychology) by the use of postulational technique is valuable if proper care is taken in the application of this method. If the method is handled carelessly and attention paid merely to externals, then psychologists fall victim to the same trait for which they criticized philosophers.

Postulational Psychology and Empirical Verification

Any psychological system in postulational form will have a great number of theorems derived from its axioms. These theorems must of course, include all facts known, *i.e.*, all experimental data available and will include many propositions stating facts not yet verified experimentally. A system which omits any of the known experimental data is (1) incomplete in the sense defined, (2) not wide enough. If it is incomplete in the sense that the given fact *F* is meaningless on the basis of the axioms set up, then clearly a new set of axioms is necessitated. If it is not wide enough in the sense that the fact cannot be derived from the given set of axioms then (*a*) either we need another axiom or (*b*) the fact *F* is itself an axiom that must be added to those we already have.

It must be clearly kept in mind however, that the failure of an experiment or set of experiments to verify any given theorem does not in itself nullify the system if the system has been demonstrated to be *consistent.* If the system has been demonstrated to be consistent and one or more of its consequences are not verified by a given experiment the possibility remains open that the conditions under which the experiment was performed differ from the conditions under which the consequence was drawn. One must not be too quick to reject a set of postulates which do not give consequences in accordance with experimentation. Suppose a set of postulates $f_i(p)$ imply a consequence $f_i(p_1)$, but actual observation shows that in many cases what we get is $f_i(q, p_1)$, *i.e.*, the presence of a factor q is observed in our experiment. We are then faced with a number of possibilities. (1) Our postulates $f_i(p)$ are not an adequate description of the situation, *i.e.*, we need one or more postulates having q as a factor. This is clearly recognized by Hull in what he called the successive approximation procedure in the correction of postulates (**6, 7**). (2) Our experiments are not adequately controlled so that q is not eliminated. (This element is overlooked by Hull.) In considering therapeutic measures, we note that those measures should be adopted which will eliminate q (assuming that $f_i(p)$ is the 'healthy' or 'normal' state of affairs).

Care must be taken not to dismiss $f_i(p)$ as 'psychological metaphysics' (**6**, p. 499), when it might give rise to invaluable investigation. As Adams (**1**, pp. 215–6) points out, we have here a hypothetical destructive syllogism. But Adams along with others forgets the interpretation of the minor premise "Under conditions A event a does not occur," and its possible relations to the deduction of the fact a from the postulates describing the conditions A.

As Hull seems to recognize, mere empirical verification in a given state of science does not necessarily verify any given system. Suppose we have two systems S_1 and S_2 descriptive of a given set of the phenomena P. Let us further assume that at moment T_1, S_1 better conforms in its theorems to *facts.* This means that S_1 is preferred to S_2 as a system.

But nothing prevents the discovery of a fact F_1 at time T_2 which will throw new light on the original set of facts so that S_1 no longer satisfies the facts and S_2 does. Thus at T_2, S_2 is preferred to S_1. Such a state of affairs actually existed at the time when Copernicus first stated his heliocentric hypothesis. At that time the evidence (*scientific!*) favored the geocentric hypothesis. This should make us cautious in discarding alternative systems on the grounds that we have no experimental verification of our theorems.

In systems where an experiment directly contradicts a theorem, as in the case of the difficulty peak of a rote series—theorem XI in Hull's miniature system (**6**, p. 5), the proper conclusion to draw is that the system is *inapplicable*. The system might still serve as an aid in comparative studies of psychological theories.

Some Remarks on the Systems Given by Hull

I should like now to analyze Hull's systems as given, recognizing however, two facts. First my remarks above and criticism of the systems themselves should not obscure Hull's main thesis. This, as Hull states, is ". . . whether we shall be able with postulates of any kind whatsoever to develop verified deductive systems" (**1**, p. 220). This is a thoroughly sound thesis. Secondly, the criticisms to be made are not to be construed as annihilating the system but rather as pointing out places where the system needs improvement.

Since the system in *Mind, Mechanism and Adaptive Behavior* is the briefer one, I shall speak in terms of this system. For the sake of brevity, I shall refer to this system (which occurs on pp. 15–28) and is entitled "Adaptive Behavior") as S_{AB}.

S_{AB} is composed of 18 propositions called 'Definitions,' and six propositions called 'Postulates.' These 24 propositions might be called the postulate set for S_{AB} (let us call this A_s).

Now, any system whatsoever is a language in terms of which certain events and objects are interpreted (**3**). Therefore, any psychological system, and S_{AB} in particular, is a language. Being a language, S_{AB} must state clearly the

object and events with which it is concerned (*i.e.*, its vocabulary) and the rules in accordance with which the sentences of the system can be transformed into other sentences either of the same or another language. These latter rules will be called, following Carnap, the *Syntax* of the system. The syntax of S_{AB} should therefore tell us how to translate the sentences of S_{AB} into other sentences of S_{AB} (these are the theorems) and into sentences describing *actual experimental operations*. (These are called *protocol* statements.)

The vocabulary of any system is always a set of *signs* for the objects and events with which it is concerned. A system can be viewed, therefore, in two ways (1) from the point of view of the vocabulary and the rules transforming sentences of the system into other sentences of the same system (2) from the point of view of the sentences and their translation into protocol statements. (1) will give us pure theory. (2) will give us pure application. The combination of the two gives us what we call *scientific systems*. Obviously since in both (1) and (2) the same system is under consideration and since we are dealing with *any* system whatsoever, it follows that any system whatsoever is at least theoretically capable of being transformed into a scientific system. Two systems S_B and $S_{\text{non-}B}$ dealing with the *same objects* are both capable of becoming (at least theoretically) scientific systems, *i.e.*, their sentences can be reduced to protocol statements. Since protocol statements are experimental operations, any observer can determine whether or not the protocol statements of S_B and $S_{\text{non-}B}$ are identical. If they are, then the question whether *B* or non-*B* *exists* is a pseudo-question. Different words are being used for identical objects. The behaviorist and non-behaviorist controversy may in many respects be resolvable into such a pseudo-issue.

The choice between two systems would lie in the completeness of the systems. If in one system, one could construct a sentence meaningless within that system yet obviously meaningful (say as translated into the protocol statements of the other system) then that first system would be undesirable. This is what Hull has in mind when in his discussion of the

existence of *consciousness* he asserts that his own system S_{AB} does not deny it but that he has found no theorem which necessitates its introduction (7, pp. 30–31). This means that Hull feels his system complete with respect to *behavior*. Furthermore, he seems to feel that any system which introduces 'consciousness' could not be complete. As is clear from the above remarks, Hull is wrong, however, when he says that the development of a system using 'consciousness' as a postulate would involve the introduction of this entity into the body of behavior theory. It would still be necessary to show S_{AB} non-complete.

These remarks are sufficient to show the weakness in a system which does not demonstrate its completeness. S_{AB} offers us no proof of the completeness, consistency or independence of A_1.

Let us consider the first postulate:

"1. The adequate stimulation of a sense organ initiates within the organism a neural reverberation which persists for some time after the stimulus has ceased to act, the absolute amount of reverberation diminishing progressively to zero, but at a progressively slower rate (stimulus trace)" (7, p. 16).

We find in this postulate the following terms: 'adequate stimulation of a sense organ,' 'organism,' 'neural reverberation,' 'absolute amount of reverberation.'

Let us consider 'adequate stimulation of a sense organ.' The postulate is either a definition of this term or it is not. If it is then it should have been listed as such and it becomes necessary to demonstrate the existence of this 'neural reverberation' which varies as stated. As a postulate, on the other hand, the sentence should describe a state of affairs. No protocol statement defining 'adequate stimulation' is given however. So that on the basis of this postulate we are left in doubt as to what operations to perform in order to achieve an '*adequate stimulation*.'

The next term to be considered is 'neural reverberation.' It might be argued that 'neural reverberation' is a term defined in bio-physics or physiology. *But*, since here too no protocol statement is given, 'neural reverberation' is no

different from 'state of consciousness.' After all, a neural reverberation is *internal* to the organism. As such it is not open to the observer directly. The neural reverberation then can be detected (1) either in terms of the behavior of the organism or (2) in the electric current along the nerve fibers. Either of these methods, however, leads to a protocol statement—which seems to be lacking in Hull's own setup. If, therefore, we replace 'neural reverberation' by 'state of consciousness' and use either or both of the protocol statements to define the term, then clearly Hull's system is as much a system in terms of consciousness as in terms of mere reaction (**5**).

This is not an argument for the introduction of the term 'consciousness' into Psychology. It is rather an attempt to show how the analysis of the language of Psychology can lead to the exposure of pseudo-problems. No matter what terms are used if the terms are adequately defined in protocol statements a sound scientific system can be constructed. It is when the terms do not have protocol definitions that they are taken as something real in themselves and the disputes begin.

Similar analysis of the other terms in the remaining definitions and postulates can be made but need not be gone into here.

Scientific Systems as Such

Enough, however, has been said specifically about postulational systems and Hull's use of the system in Psychology. We should consider now the question of the nature of scientific systems in general in order to show that their form is postulational—and secondly that the construction of systems is essentially a philosophic task (**8, 12**).

The first remark to be made is that no *science* exists without some form of unity. A mere aggregate of facts is never sufficient to constitute science.

The fact of glandular disturbance, the fact of certain emotional disturbance; the fact of hunger, the fact of seeking food, the fact of success in running through mazes—all of these taken as mere facts obviously do not give us science.

Science as a construct of the human mind, *i.e.*, as a body of propositions, achieves its unity by the inter-relation of these propositions. But every proposition expresses a *fact*—and in particular, a proposition in a system of psychology expresses a fact about psychological processes. (These *facts* are given in the protocol statements of the propositions.) This means that every proposition which is meaningful must express or be expressible in, one or more protocol statements. It follows therefore that propositions *express* the inter-relations of facts, *i.e.*, the inter-relations of events and sets of operations which the scientist can directly experience and perform. It follows from this that the unity of science expresses the unity of actual events.

Reversing our analysis we see that the 'order and connection of events' gives us the possibility of having a *system* of science. Even more, the inter-relation of events makes absolutely essential that a science, to be a science, be a unified and inter-related body of propositions. *A postulational system meets this first condition.*

The next step is to determine the type of inter-relationship among the propositions. Propositions may be related either essentially or non-essentially. Propositions are said to be related non-essentially if the only relationship between them is identity of subject-matter. Thus the two propositions "The rat can be taught to pull in food by string pulling" and "The rat is a small animal with four legs" would be propositions so related. Two propositions are said to be *essentially* related if (1) either implies the other, (2) both are implied by the same set of given propositions. Illustrations of these are sufficiently copious not to need citation. Where we have (as in practically all cases) more than two propositions, then they would be said to be essentially related if either or both of the two conditions of implication were satisfied. Both of these cases are of course deductive relationships between the propositions. In case (1), that one which implies the other is either a given proposition, *i.e.*, a postulate or itself a deduction from other propositions. If the latter, then these other propositions are again either

postulates or consequences. This continues until we reach a set of propositions taken as postulates. The same holds true of the set of given propositions in case (2). Both cases then result in a form like our illustrative case above—*i.e.*, in the form of a postulate set. The inter-relation among the propositions is then that of 'implication' or 'deduction.' Thus a scientific system has as its form the postulational system which we have described above. The study of properties of postulate sets is the study of the formal properties of scientific systems.

Having seen that the postulates or given set of propositions of a science state a set of facts, it remains to be shown that the implications of the set of postulates also assert facts. To demonstrate this, we need two logical principles:

1. If p and q are propositions, then $p \cdot q$ is a proposition (*i.e.*, the simultaneous assertion of p and q is also a proposition).

2. If p implies q, then the simultaneous assertion of p and not $-q$ is impossible (*i.e.*, $p \rightarrow q \equiv pq' = 0$).

As we saw, to each proposition there corresponds a fact or event. Then if one proposition p is shown to imply another q, this means that the fact corresponding to q is in some way related to the fact corresponding to p. If now we use the principle $p \rightarrow q \cdot \equiv pq' = 0$. Then the implication of a q by p results in a new proposition pq' which is not possible. That is to say, the state of events corresponding to the simultaneous assertion of p and not $-q$ (*i.e.*, the simultaneous occurrence of event p and event not $-q$) cannot take place. The role of experiment is then to determine whether pq' does or does not take place. In other words the postulate system offers precise experimental problems. This also makes evident the startling fact that if it were possible to achieve a completely adequate set of postulates—adequate in every way—experimentation would, as Eddington has pointed out, become unnecessary and have the same relation to the sciences as the figure in Geometry has to that science.

If, then, we accept the definition of Philosophy as a rational systematic description of reality, science is seen to be gradually approaching philosophy.

Conclusions

Hull's thesis is given an epistomological foundation and seen to be correct. His own use of the postulational technique, however, is far from adequate and overlooks a number of very important features involved in postulational technique.

We have incidentally shown the plausibility of the principle that a scientific system is possible because that which science studies—which we call Nature—is systematic in character.

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COMMENT ON THE FORMATION OF DISCRIMINATION HABITS

BY T. L. McCULLOCH

Yale Laboratories of Primate Biology

I

Some five years ago, while studying individual differences in the rat (7), the writer was embarrassed not only by inability to devise a satisfactory method for determining reliability of learning scores, but also by the fact that evidence for the validity of scores on discrimination habits was extremely scant. Furthermore, Lashley's now oft-cited observation that "The form of the learning curve . . . suggests that the actual association is formed very quickly and that both the practice preceding and the errors following are irrelevant to the actual formation of the discrimination" (6, p. 135) was alarming to one who took seriously the problem of individual differences.[1] Krechevsky, in common with other investigators, had recently noted (1, 2, 3) the tendency of rats to exhibit more or less consistent forms of behavior in the discrimination situation, and had made a distinctive contribution in devising techniques for analyzing these behavior patterns which he designated by the term 'hypotheses.' These 'hypotheses' were noted as being 'systematic,' 'purposive' or 'docile,' 'abstractive' or selective, and in part initiated by the organism; they might be 'adopted,' 'tested,' 'brought to perfection,' 'given up' in favor of others. Now, if it were true that an organism could adopt 'hypotheses' willy-nilly, or on the basis of processes beyond the influence of ordinary association, the practice of using total scores (trials, errors) to represent ability might not be justified. Obviously, if an animal is learning nothing in respect to the

[1] Fortunately, his observations did not deter Lashley from the labor of studying the effect of cerebral lesions upon discrimination habits.

final habit while it exhibits irrelevant 'hypotheses,' there may be no point in using total scores to represent learning ability. There would, however, be justification in using total scores if an animal were being differentially affected by the cue stimuli while it is both responding with chance frequency in respect to those stimuli and at the same time is showing systematic responses which are irrelevant to the discrimination response finally learned.

According to association theory, all stimuli acting at the time of response become associated with that response. Hence, differential training should have a differential effect on each trial, from the very beginning of training, even though the response which occurs over a series of trials is not economical with respect to the cue stimuli. This statement of course presupposes that conditions are so arranged that the relevant cues actually are effective stimuli, *i.e.*, affect the sensorium, and that the associations formed in early training with these stimuli are similar to those upon which the final habit is based. Further, according to this formulation, cumulative learning should occur even while an animal is exhibiting 'hypotheses.'[2] The hypothesis that the formation of a discrimination habit is a cumulative process thus derives directly from association theory. It remains to adduce experimental evidence which will test the hypothesis, and particularly will test the corollary or special hypothesis that early training has a differential effect upon an animal at a time when increased economy is not evident in respect to training stimuli and even when systematic response irrelevant to the training stimuli is evident.

In order to test both the validity of scores of a discrimination habit and the applicability of the continuity hypothesis in a presumably typical discrimination situation, McCulloch and Pratt (8) gave three groups of rats different amounts of training on a weight discrimination habit and upon reversal of cues found negative transfer roughly proportional to the length of training upon the original habit as measured in

[2] Spence (9) has given a theoretical account in some detail in stimulus-response terms of how learning may occur while 'hypotheses' are being exhibited, and has even shown that 'hypotheses' would be expected in the typical discrimination situation.

errors. One group received but 28 trials before reversal; with one possible exception members of this group showed no preference for the positive stimulus and exhibited consistent position responses in the original training. The results were taken to indicate that, to the extent that the weight discrimination situation is representative of discrimination situations in general, (1) total scores may validly be used to represent ability to acquire the habit, and (2) the hypothesis is substantiated that the formation of the discrimination habit is a cumulative process. It should be emphasized that a cumulative effect occurred in early training even while an animal was exhibiting 'hypotheses.' The linear relation of errors before reversal and excess errors after reversal may have been a function of the particular technique employed. The significant point is that in some degree a small amount of training had a differential effect. It should be emphasized also that the rats were forced from the very beginning to make the proper muscular adjustment to the weights and hence to receive differential stimulation.

II

Krechevsky (**5**) has recently presented evidence in this journal which he interprets as inconsistent with the continuity hypothesis. To appreciate Krechevsky's interpretation of his results it is necessary to examine critically his formulation of the continuity hypothesis, or 'assumption,' as he calls it. According to his formulation, a correct response results in an increase in the tendency to approach the positive stimulus and an incorrect response results in a decrease in the tendency to approach the negative stimulus. These increments and decrements occur at the very beginning of training and are constant in amount throughout the learning process. In the hypothesis, he notes, "Nothing is said about the 'attention' of the animal, nothing about the 'awareness' of the animal with respect to the important stimuli" (**5**, p. 111). The above statement, it is believed, is a fair representation of Krechevsky's formulation of a continuity hypothesis. Of course an investigator has the

privilege of stating a hypothesis as he sees fit. The significance of his formulation, however, depends upon the importance of the theory upon which the hypothesis is based. The immediate present purpose is not to discuss the theory, if such there be, upon which the above formulation is based, but simply to show how Krechevsky's formulation of a continuity hypothesis is not in line with the prevailing interpretation of association theory.

Although some of the differences between Krechevsky's formulation of a continuity hypothesis and one based upon association theory are probably already apparent, it may be worth while to make these differences explicit. First, it should be noted that according to association theory, *increments and decrements in response tendency should occur from the very first only if the relevant stimuli so affect the sensorium that the associations formed are similar to those upon which the final habit is based.* Types of apparatus and procedure vary in the extent to which this important condition is met. As stated above, in the weight discrimination study of McCulloch and Pratt the rats were forced from the very first to receive differential stimulation. With other discrimination techniques experimenters ordinarily give their subjects preliminary training with neutral stimuli, which procedure results in such orientation that differential stimuli are from the beginning more or less effective. If the habit of orienting fairly uniformly toward stimuli is not established in preliminary training, at least it is established in the first part of differential training. Only when the relevant cues are effective will associations with them be formed. Spence has recently treated the problem of orientation toward visual stimuli as follows:

> . . . the animal learns many other responses in addition to the final, selective approaching reaction. Prominent and important among these are what have been termed, for want of a better name, 'preparatory' responses. These latter consist of the responses which lead to the reception of the appropriate aspects of the total environmental complex on the animal's sensorium, *e.g.*, the orientation

> and fixation of head and eyes towards the critical stimuli. That is, the animal learns to 'look at' one aspect of the situation rather than another because of the fact that this response has always been followed within a short temporal interval by the final goal response. Responses providing other sensory receptions are not similarly reinforced in a systematic fashion and hence tend to disappear (10, p. 432).

It is difficult to determine to what extent the above discussion bears upon Krechevsky's statement that the continuity hypothesis has naught to say about 'attention' and 'awareness' with respect to the cue stimuli. If Krechevsky uses these terms to refer to the same phenomena described by Spence and the writer, his formulation of the continuity hypothesis must differ significantly in respect to these phenomena from that derived from association theory.

Krechevsky's statement that increments and decrements in response tendency are constant throughout the learning process does not necessarily follow, in our present state of ignorance about learning curves, from association theory. It is unfortunate that he classes Spence among those who assume that increments and decrements remain constant, for, in the article to which Krechevsky refers (9), Spence very explicitly postulates that these increments and decrements vary according to certain formulæ, and presents tables showing how these variations affect hypothetical cases of learning. However, as far as the amount of transfer to be expected upon reversal of cues is concerned, it is irrelevant whether increments and decrements remain constant in amount or vary throughout the learning process. No matter whether increments and decrements remain constant in amount or vary according to a continuous function, negative transfer after reversal should be proportional to original training.[3] Whatever may be the shapes of learning curves,

[3] The terms 'increments' and 'decrements' in response tendency are used only for the sake of exposition. It may be emphasized that these increments and decrements are constructs, not data. Actual behavior observed is a resultant of inferred response tendencies. As the constructs have been used, increments and decrements may differ in the ways in which they vary with respect to the strength of excitatory tendency.

it follows from association theory that, when training is given with conditions such that cues are effective stimuli, associations are formed so that upon reversal of cues a certain amount of negative transfer results.

III

Using the Lashley jumping technique, Krechevsky trained one group (Group II) of rats for 20 trials, another (Group III) for 40 trials, and studied the effects of these amounts of training upon the reversal of a visual discrimination habit. The effects were determined by comparing scores of Groups II and III after reversal with those of a control group (Group I) which was trained throughout on the same habit to which Groups I and II were reversed. Briefly, comparisons revealed 4 notable results: (1) Group II learned more rapidly than did Group I. (2) Group II learned at approximately the same rate as did Group I after the latter's first 20 trials. (3) Group III learned considerably faster than did Group I. (4) Group III learned significantly more slowly than did Group I after the latter's first 40 trials. These results hold consistently, though with varying degrees of statistical significance, whether trials to mastery, total errors, initial errors, or repetitive errors are used as criteria. The bearing of these results upon the continuity hypothesis derived from association theory will now be considered.

In analyzing Krechevsky's results, the following assumptions will be made: First, training on the two stimulus

In one instance (Spence, 9) it has been assumed that decrements vary discontinuously with respect to strength of excitatory tendency. Partly on æsthetic grounds the writer prefers a different set of constructs. Instead of speaking of response decrements, he would prefer to emphasize increments of response (avoidance) in respect to the negative stimulus. This shift of emphasis arises from the interpretation that the negative stimulus becomes conditioned to a new response, incompatible with approach, as contrasted with the notion that Pavlovian inhibition is built up in regard to the negative stimulus. In line with this interpretation, increments in response tendencies with respect to positive and negative stimuli should vary *continuously*, and the changes in response tendencies for the two stimuli should vary in the same manner, *i.e.*, curves of the same type would be expected. The slope of the curves would be a function of 'reward' and 'punishment' features of the situation, *e.g.*, the slope for the increment with respect to the negative stimulus should be great with shock, and small with mere blockage of response (*contr.* Spence, 9, pp. 431–432).

situations involved the formation of a response common to both, identifiable as a decrease in tendency to make repetitive errors, and the formation of this response occurred predominantly in early training. Second, during the first 20 trials of training the animals did not so orient toward the cue stimuli that any associations were established which were similar (for Group I) or directly antagonistic (for Groups II and III) to those upon which the final response was based. Third, during trials 21 to 40 the animals sometimes, but not always, *did* so orient toward the cue stimuli. If these assumptions are accepted, Krechevsky's results are obviously exactly what would be expected according to the continuity hypothesis of association theory.

It must be frankly noted that the accessory assumptions in the above analysis are not derived in detail from association theory. Certainly there is nothing in the theory which would enable one to deduce, with the particular animals, general conditions and techniques employed, just how much positive transfer would result from a reduction in repetitive errors, or how much training would be required before the animals would so orient that associations formed with cue stimuli would be similar to those upon which the final habit was based. These assumptions should simply be considered necessary for an interpretation of the data if the results are to be considered intelligible according to association theory.

IV

From the point of view of this discussion, Krechevsky has shown that, with the particular subjects, apparatus and experimental techniques employed, associations with relevant stimuli were not formed at the very beginning of training and that other associations were formed. The bearing of these results upon the validity of using total scores before mastery as an index of ability seems obvious. If the period before associations with relevant cues are formed is constant, or if it varies insignificantly with different subjects or directly with the amount of training necessary for formation of the habit, the use of total scores may be justified for practical

purposes. Usually experimental control can be introduced to make this period an insignificant minimum, or, as with weight discrimination, zero.

Let us now turn to Krechevsky's interpretation of discrimination learning, which he considers to be supported by his data. According to his 'non-continuity' assumption, ". . . once an animal is immersed in a given problem-situation the animal selects out of the welter of possible stimuli certain sets of discriminanda to which he reacts. Eventually he gives up responding to his first set of discriminanda and responds to another set, and another set, etc., until he begins to respond to the relevant set. From then on, and from then on only, is he learning anything about the discrimination involved, or, from then on only are his 'bonds' being strengthened, etc." (5, pp. 111–112). In his summary, Krechevsky states: "In the course of responding with these various 'irrelevant' hypotheses during the pre-solution period, the reinforcements and non-reinforcements of the animal's responses have little residual effect in determining the animal's behavior *as far as the relevant discriminanda are concerned.* It is suggested that only when the animal begins to 'pay attention to' or 'react to' the important stimuli does reinforcement begin to have any effect" (5, p. 132).

The terms 'react,' 'respond' and 'pay attention' may be interpreted in two very different ways. According to the first interpretation, when an animal 'reacts,' it responds systematically with either greater or less than chance expectancy to a certain stimulus. 'React' is thus synonymous with 'hypothesis' as far as the systematic nature of the latter term is concerned. If this interpretation is applied to Krechevsky's final suggestion, reinforcement occurs only when an animal responds systematically to the relevant stimuli. This last clause has the form of a thoroughly respectable scientific hypothesis: it is subject to disproof. In view of the above quotations and other material to be presented it is possible that Krechevsky champions this hypothesis.

According to the hypothesis, if negative transfer results

upon reversal of cues in a discrimination habit, systematic response to the relevant stimuli must occur before reversal. In another recent article (4, p. 103), Krechevsky accounts for the negative transfer obtained by McCulloch and Pratt after only 28 trials before reversal, by *assuming* that the animals were responding to two 'hypotheses' before reversal, one presumably being on the basis of weights. In this treatment he cites material gathered earlier by him showing that rats sometimes respond systematically over a series of trials to two sets of stimuli; in his illustration they responded with more than chance frequency to position and light. But as a matter of actual fact, as far as error scores indicate, the rats of McCulloch and Pratt did not, with the possible exception of one animal, respond systematically to weight stimuli before reversal. Krechevsky ignores this fact.

According to the above hypothesis Group III of Krechevsky's recent study, since it showed negative transfer upon reversal of cues, should have responded systematically to the relevant stimuli before reversal. However, from the evidence it may be presumed that as far as error scores are concerned Group III did not so respond. Had the animals done so, Krechevsky seemingly would have indicated the fact. Although no record of errors before reversal is given, an analysis of Krechevsky's tables reveals that during trials 21 to 40 the control group averaged .494 initial errors per trial, which is as near chance expectancy (50 per cent) as one could wish. Presumably, Group III also did not depart far from chance in the same period of its original training.[4] In accounting for the negative transfer of Group III, Krechevsky assumed that the animals responded to the relevant stimuli. It is noteworthy, particularly in view of the fact that he earlier applied statistical techniques for the detection of systematic behavior, that Krechevsky neglected to use the best indicator

[4] The logical possibility is ignored that the chance score of the group average is derived from scores of some individuals who responded with greater than, and others with less than chance expectancy in respect to the cue stimuli. It should be a distinct contribution if it could be shown that appreciable negative transfer results when the average is so composed.

available in this case and merely *assumed* that the animals were responding (systematically?) to the cue stimuli.

In discussing why Group II should not show negative transfer after only 20 trials before reversal Krechevsky states, "All this assumes, of course, that the animals of Group II had not already completed their 'preliminary hypotheses' and started on their final hypothesis within the first twenty trials" (**5**, p. 116). It is difficult not to infer, especially in view of his earlier explanation of McCulloch and Pratt's results, that the negative transfer of Group III was attendant upon their having adopted 'hypotheses' in respect to the relevant cues before reversal. The fact is very pointed that Krechevsky does not explicitly state what has been inferred but instead speaks of Group III as having begun to *react* to the relevant cues. This fact suggests an alternative interpretation of his meaning of the term 'react.'

According to a second interpretation, an animal 'reacts,' 'responds,' 'pays attention' to relevant stimuli *even in the absence of systematic behavior* when it forms associations with the stimuli. One criterion of the formation of associations is negative transfer upon reversal of cues. If, by definition, an animal reacts to stimuli when associations are formed, Krechevsky's final suggestion to the effect that ". . . only when the animal begins to 'pay attention to' or 'react to' the important stimuli does reinforcement begin to have any effect" (**5**, p. 132) obviously holds. One would not expect Krechevsky to accept this definition of 'react,' however, for with such acceptance his statement could be paraphrased to read "reinforcement occurs when associations are formed," or simply "reinforcement occurs when it occurs."

Regardless of what Krechevsky's interpretation of discrimination learning may be, the fact is that the results of McCulloch and Pratt, and seemingly those of Krechevsky with Group III, indicate that rats form associations in the absence of detected systematic performance in respect to the relevant cues. Moreover, it should be emphasized that the non-occurrence, during early training, of reinforcement of the final response to be learned is not inconsistent with association

theory. Finally, the use of the terms, 'react,' 'respond,' and 'pay attention' as an explanation of the occurrence of the formation of associations, seems, in the absence of descriptive attributes of those terms, gratuitous.

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A CORRECTION TO 'A CRITICISM OF HULL'S GOAL GRADIENT HYPOTHESIS'

BY JACK BUEL

Wesleyan University

In the article, 'A Criticism of Hull's Goal Gradient Hypothesis,' which was published in the PSYCHOLOGICAL REVIEW, 1938, **42**, 395–413, there appeared on page 396, through a regrettable inadvertence, a misleading statement concerning the goal gradient hypothesis.

In fairness to Professor Hull as well as to the readers of the paper, it should be made clear that the goal gradient hypothesis is a principle distinctly separate from the theorems derived from the action of the principle in specific maze situations. The goal gradient hypothesis states that "the goal reaction gets conditioned the most strongly to the stimuli preceding it, and the other reactions of the behavior sequence get conditioned to their stimuli progressively weaker as they are more remote (in time or space) from the goal reaction" (p. 26).[1]

The occurrence of a gradient of maze responses is, according to Professor Hull, to be expected as a logical consequence from the goal gradient hypothesis (*op. cit.*, p. 35). Because of the quantitative characteristics of this hypothetical gradient it is deduced, for example, that the discrimination of distance by the rat should conform to Weber's law. By similar reasoning it is deduced, other things equal, that blind alleys should be more easily eliminated near the goal than at some distance from the goal; *i.e.* that, except where over-ridden by opposing factors, more blind-alley entrances should be made at the beginning of the maze than at the end.

[1] The goal gradient hypothesis and maze learning. PSYCHOL. REV., 1932, **39**, 25–43.

It is the sincere hope of the writer that this correction will serve to remove the unintentional confusion of the goal gradient hypothesis with the theorems based upon it which seek to explain certain phenomena of learning such as the order of blind difficulty in the maze.

A REPLY TO DR. RAZRAN ON THE TRANSPOSITION OF RESPONSE IN DISCRIMINATION EXPERIMENTS

BY KENNETH W. SPENCE

State University of Iowa

In a recent article (6) Dr. Razran, who has contributed invaluable service to the non-Russian reader interested in conditioned reflexes, has made certain criticisms of my theoretical formulation of transposition in discrimination experiments (7). While reluctant to use up journal space to straighten out the misunderstandings which have arisen, it is especially important, in view of Razran's reputation as an authority on conditioning, that the matter be clarified. Razran made four specific criticisms:

(1) The first seems to be based on the fact that restriction of the extent of generalization begins very early and is progressive. From this fact Razran reasons that the range of transposition in discrimination experiments should likewise exhibit progressive restriction with training. It should first be made clear that I did not deal with this problem in my paper. The only assumption I made that is related to this fact of progressive restriction of generalization was that at *the conclusion of learning* there is generalization of the positive and negative tendencies to the adjoining stimuli. This is a reasonable assumption for there is no evidence in the conditioning literature that the progressive restriction ever becomes complete. Indeed, Hovland's recent data (4) shows that the restriction with training is very limited.

But what of Razran's implication that the range of transposition in discrimination experiments should become restricted because of the restriction in the range of generalization? Such an unqualified claim reveals a failure to take into consideration the complicated nature of discrimination behavior. The theoretical structure I have proposed simply

does not and cannot make any such prediction. To do so would neglect innumerable other factors which are inherent in the discrimination setup, *e.g.*, the interaction between the various competing stimuli, the absolute degree of restriction and the shape of the generalization curves, the distance of the test stimuli from the original training pair, etc. When one takes into consideration all of these factors, it is possible to state with some degree of precision the course of transposition under different conditions of training and testing. Space limitations permit me to state only that the course of transposition does not coincide with Razran's statement.

Quite apart from this, Razran seems to err in his statement that the results of my own experiments contradict his assumed implication. He is evidently referring here to the fact that in the successive test series, after learning was completed, the results showed greater transposition instead of less as he would have expected. But Razran seems to have entirely lost sight of the fact that *in these test series the test stimuli are always reinforced*. After such direct reinforcements of these stimuli, it is not surprising that one does not subsequently find greater restriction of generalization. The relative strengths of the stimuli are entirely altered.

(2) The first part of Razran's second criticism does not deal with the main postulates of my theory. It is concerned with one of two alternative assumptions I suggested in order to account for the fact that the extent of transposition in the direction of smaller stimuli after training to the smaller stimulus is much less than to larger stimuli after training to the larger. This assumption, that the range of generalization is less with stimuli of smaller magnitude, was merely a *logical possibility* which I offered. Just how the writer could have been expected to be acquainted with the contrary evidence of Hovland's study, which appeared some months after mine, it is difficult to see. The Pavlovian data available in English on such a refined quantitative point are quite inadequate. It so happens that I have long since abandoned this possibility. Thus, I hope, I have set at rest certain fears that I suspect Razran harbors as to the inflexibility of such theories and theorists.

Concerning the shape of the generalization curves I employed, I stated very specifically that my selection was purely arbitrary. A survey at the time of writing of the available evidence as to the shape of the generalization curves revealed no evidence definitely against the curve I adopted. Indeed, Bass and Hull's study (1) showed positive acceleration for their excitatory curve, although it was plotted arbitrarily against four equidistant abscissae values, and when certain corrections were made, their inhibitory curve likewise showed positive acceleration. Hovland's negatively accelerated curve (3) appeared while my paper was in press. Upon examining the data on which it was based, I discovered that, if median instead of mean values were used, the first part of the curve was positively accelerated. With no evidence definitely refuting the positively accelerated curve, I decided to retain it. Even at the present time, there is little or no justification for Razran's statement to the effect that my "hypothetical curves are distinctly different from what experimental data would warrant" (6, p. 535). As he himself states, the curves of generalization appear to be specific to particular continua. I should like to add that for some time I have been using an ogive shaped curve with certain other refinements of the inhibition curves based on the fact that the excitatory processes are not equal for all stimuli.

(3) In his third point, Razran seems to think that I have neglected—and he kindly excuses me on the basis of the unavailability of the information to the non-Russian reader—the facts of relational and pattern conditioning. There is revealed in this part of his criticism a further unfortunate misunderstanding of my theoretical formulation. I simply do not believe that patterning is involved in the type of discrimination problem with which we are here concerned. Hence, I have not assumed patterning to be operating. By this statement I do not mean to imply, however, that patterning is not important under certain conditions of discrimination. It is, but as in conditioning experiments, the conditions determine whether or not patterning is involved. On the other hand, one can proceed, if one wishes, to develop a theory

which does assume that patterning is operating. Gulliksen (2) and Hull (5) have each developed modified theories which involve different aspects of patterning. As yet it is too early to see if patterning is necessary for transposition phenomena. I shall not hesitate to modify my theory if such turns out to be the case.

(4) The final point needs only brief discussion. Without claiming to champion the Gestalt psychologists, Razran argues that simple conditioning is not the only possible generator of scientific psychological principles and that a relational concept is 'just as fruitful in testable logical hypotheses.' Apparently, he failed to read the footnote to my final paragraph, which shows that we are not altogether in disagreement on this point. I repeat it now in the text. "The writer does not wish to deny the possibility that a theory of this type (Gestalt) may be developed which will be capable of accounting for these experimental facts satisfactorily. Such dogmatic denials have no place in the realm of science. At the same time, however, we would emphasize the fact that the burden of the proof devolves on the theory. Its only claim to consideration lies in the extent to which it leads logically to consequences which coincide with empirical events" (7, p. 443). It still remains to be seen, however, whether the Gestalt relational concepts will be fruitful in this field. In the meantime, I would not in the least discourage their use by those who prefer to employ them.

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